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The Jackson Lecture.[1]

AUSTRALIA'S CONTRIBUTION TO OUR KNOWLEDGE OF INSECT-BORNE DISEASE.

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IT is an honour to be invited to give this lecture to commemorate the name of Ernest Sandford Jackson, who was a man of parts and an erudite historian. It is an honour one could not civilly refuse, yet it filled me with trepidation, for I am no historian. I have had, therefore, to choose my subject, not because of its intrinsic historical interest, but simply because it is one in which I have had occasion—as has any worker in any field of research—to examine the literature and endeavour to weigh it. This literature has interested me intensely. It contains some of the most remarkable life-histories and adaptations in the whole of biology; it presents a fascinating variety of epidemiological situations; and it shows clearly how research methods have grown from the acute yet simple observations of the early naturalists, through experimental studies of increasing complexity, to the highly integrated team work of the present day. Moreover, the subject is of substantial importance to Australia, and Australians have played a useful part in building up our knowledge of it. I hope, therefore, that it will not be thought too specialized to interest a general medical audience.

Before embarking on my subject, let me endeavour to define it a little more precisely. First, it is necessary to make the perennial apology that by "insect" is meant "Arthropod", and yet not all Arthropods, for *Cyclops* as a vector of the Guinea worm, for example, fortunately does not intrude on the Australian scene. Ticks and mites do, however, and they should not escape attention. Secondly, I would not limit the infections to be considered to those in man, but would include at least some of the lower animals, for this a field in which comparative pathology is a very real thing, and any artificial distinction between "medical" and "veterinary" and "zoological" is an impediment to progress and understanding. Thirdly, for the purposes of the discussion, I would accept as "Australian" anyone who has done a substantial amount of work in this country. Joseph Bancroft was born in Manchester and T. L. Bancroft in Nottingham, but both lived their lives and did their work here. Hamilton Fairley left Australia, but has done more than enough in and for the country for us still to claim him as ours. Sir David Bruce, of sleeping sickness and Malta fever fame, was also a Victorian. He, however, was taken to England at the age of five, so, reluctantly, we must leave him out.

Science is international. Its results belong to the world (or they should!); but that is no reason why we should not take pride in those of our community who have served science well. They, though not their work, belong to us.

Classification.

Infections of vertebrates with which Arthropods are associated fall into four natural epidemiological groups. Representatives of all have been studied in Australia, and it is convenient to introduce our subject by reviewing them briefly.

I. *Infections which are Specific to Both Definitive and Intermediate Hosts.*

Infections specific to both definite and intermediate hosts are of some zoological antiquity; they have reached stability, and the host-parasite relationship is precise. They depend for transmission and maintenance, not only on suitable climate, but on definite population densities of both the vertebrate and Arthropod hosts. There are no outside

[1] Delivered at a meeting of the Queensland Branch of the British Medical Association on September 5, 1947.

reservoirs of infection. This group is a large one, and includes: among the worms, filariasis of man and onchocerciasis of cattle; among the protozoa, malaria in man (and many other animals) and piroplasmosis of cattle; among the viruses, dengue fever in man and ephemeral fever in cattle. Yellow fever provides an interesting transition to the next group, for ordinary urban yellow fever belongs to group I, whereas jungle yellow fever behaves as a member of group II.

II. *Infections which are Normally Restricted to Arthropod and Lower Mammal, Man being Infected as an Accidental Intruder into the Cycle.*

The second is the large and important group of "reservoir animal" infections. The relationship of the parasite to the reservoir mammal and the Arthropod is much as in group I, but not quite so restricted. When it does slip over into a human host it is usually ill-adapted and the disturbance correspondingly violent. The epidemiological characteristic of the group is the apparently haphazard distribution of infections with no relationship between cases. They really are accidents. Abroad, tick-borne relapsing fever and leishmaniasis are examples; in Australia, the rickettsial infections are the most important members. The encephalomyelitis group in various parts of the world belongs here, as does, quite possibly, Australian "X disease", which may even now be lurking in some native mammal, until some chance association causes it to "spill over" again into a human community. If Bull's (1922) interesting find of *Habronema* larvæ in "bung-eye" is confirmed, this curious affection might also be classified here.

III. *Infections Transmitted Mechanically by the Arthropod.*

Specificity is breaking down in group III, and the infections form an unbroken series from surra, which is transmitted only by biting flies, through plague, which shows relationships with group II, but is not always transmitted by fleas, to dysentery and enteric, of which flies are only a casual, though sometimes important, means of spread. The epidemiological significance of the vector insect is usually much more obvious in this group than it is in group II.

It may be remarked in passing that it is fortunate and mysterious that surra has not become established in northern Australia. Horses are highly susceptible and are numerous in the north, as are *Stomoxys*, *Lyperosia* and Tabanidæ which are good vectors. Surra is widespread in the Netherlands East Indies, and Timor ponies, Zebu cattle and buffaloes (though the last may not be good hosts) were introduced in the early days of settlement. Cleland (1909) even found *Trypanosoma evansi* in a shipment of camels which had been disembarked in Western Australia. He had those destroyed in which trypanosomes were found in direct smear or by blood inoculation, but one may doubt whether that alone would be a completely effective measure. We may be reasonably certain that the disease is not present, but we do not know why; possibly some subtle population relationship, such as that which determines the incidence of malaria, may explain its absence, but that is mere speculation.

IV. *Infections in which the Arthropod Itself is the Infecting Agent.*

When the arthropod lives only on the surface, the condition may perhaps be more properly termed infestation than infection. When, however, it invades the tissues, it becomes ætiologically, pathologically and epidemiologically a problem in infectious disease (Mackerras, 1936). This group lies outside the title of the lecture, and will therefore be omitted from later sections. Two remarks may, however, not be out of place before leaving it. The first is that some very interesting work has been done on it in Australia, of which Clunies Ross's (1926) studies of tick paralysis in dogs due to *Ixodes holocyclus*, and the combined sheep-blowfly investigations of the Council for Scientific and Industrial Research and the New South Wales Department of Agriculture are the most outstanding. The second is that the sheep-blowfly provides the only instance I know of the large scale use of preventive surgery rather than preventive medicine. Susceptibility to "strike" lies in the wrinkles which are developed on the breech of merino sheep. The late J. H. W. Mules, an enterprising sheepman, devised a simple operation to remove the wrinkles; the Council for Scientific and Industrial Research developed it further, and now the incidence of blowfly strike in many flocks has been reduced by this surgical means alone to a tithe of what it was.

The First Period in Research.

And so we come at last, perhaps somewhat timorously, to our general historical review. For the sake of orderliness, we may recognize three periods in the development of knowledge, though it is necessary to emphasize that they are not sharply differentiated, but merely rather arbitrarily selected stages in a continuous stream of endeavour. The metaphor of the stream, indeed, is not inappropriate, for the first stage is like a mountain torrent, taking origin in a few clear springs of inspiration, tumbling eagerly through gorges and over obstructions, gathering fresh volume and force from every tributary; the second like the stream emerging into open country, meandering widely and perhaps rather aimlessly through a fertile plain; and the third (may we hope?) like the broad and useful river flowing purposefully, if still a little erratically, to its destination.

However fanciful the metaphor may be, the periods are convenient, so we shall use them.

The first period may be taken as including approximately the last third of the nineteenth century, and may be defined roughly as the period of observation and of simple experiments designed to make observation more easy. It was profoundly influenced by the discoveries of Darwin and Wallace, and its outlook was clearly expressed by Manson (1898) in the introduction to the first edition of his "Tropical Diseases":

> It is evident from what has been advanced that the student of medicine must be a naturalist before he can hope to become a scientific epidemiologist, or pathologist, or a capable practitioner. The necessity for this in all departments of medicine is yearly becoming more apparent, but especially so in that section of medicine which relates to tropical disease.

Zoology was in the vanguard in those years. Comparative anatomy, comparative pathology and parasitology were lusty youths sweeping all before them. Perhaps the clearest picture of the fertility of that period can be given by simply listing some of its more outstanding achievements in the field we are considering, not forgetting that there were achievements just as outstanding in other fields too.

1866. Wucherer, in Brazil, found little worms in the urine of patients with chyluria. (Hence the generic name *Wuchereria* with which students are now burdened in place of the old familiar *Filaria*.)
1872. Lewis, in India, found these larval filarial worms in the blood.
1876. Joseph Bancroft, in Australia, discovered the adult filarial worm, and Cobbold in 1877 named it *Filaria bancrofti* after him.
1878. Manson, in China, followed the early development of *Filaria bancrofti* in *Culex fatigans*.
1880. Laveran, in France, discovered the parasite of malignant tertian malaria.
1893. Smith and Kilbourne, in America, demonstrated transmission of piroplasmosis of cattle by the progeny of ticks which had taken an infective feed.
1895. David Bruce, in Africa, discovered transmission of trypanosomiasis by tsetse flies.
1898. Ronald Ross, in India, elucidated the development of malaria parasites in the mosquito.
1899. T. L. Bancroft, in Queensland, followed the complete life history of *Filaria bancrofti* in *Culex fatigans*.
1900. Reed, Carrol, Lazaer and Agramonte, in Cuba, transmitted yellow fever by *Aëdes ægypti*.

"Seek and ye shall find"! What a period of discovery that was! And what jockeyings for position there were, too, amongst those early workers! Many people were

thinking of the same things, making the same observations, and priority meant a great deal more then than it does (one hopes) to us today. Thus Cobbold (1879), in endeavouring to settle the arguments, records the discovery of the adult *Filaria bancrofti*: by Bancroft on December 21, 1876 (announced by Cobbold in *The Lancet* in July, 1877), by Lewis on August 7, 1877, by Silva Araujo on October 16, 1877, and by dos Sanctos on November 12, 1877. Bancroft was placidly outside most of this argument, because news travelled slowly in those days and was stale by the time it reached him.

The earliest workers naturally had no idea how the worms were transmitted, but Lewis's discovery provided the key, and Bancroft, in April, 1877, stated that he strongly suspected mosquitoes. He even tried experiments, but unfortunately with an insusceptible species (*Aëdes vigilax*), and so had no success. The same idea was growing in the minds of others, too, but it remained for Manson to put it to the acid test and demonstrate for the first time that a parasite of man (or, indeed, of any mammal) began its metamorphosis in an insect. Thereafter he was led astray by prevailing entomological opinion, but his discovery still stands alone for the work it stimulated. That was perhaps Manson's greatest claim. Others—the Bancrofts, Ross, Lewis, to name a few—were great workers, but Manson was the great stimulator, catalyzing almost the whole field of tropical medicine. Had he done no original work (and his contributions were substantial) his niche in the temple of fame would still be safe on that account alone. We had his like in the field of animal health in Australia in the person of J. A. Gilruth. Such men should not be forgotten.

It might be said that I have spoken largely of an Englishman when my task is to tell of Australians, but one should not lose perspective. We may therefore conclude this period by considering the joint merit in the work of an Englishman and an Australian. I have just said that Manson was led astray by entomological opinion. Ross in India and T. L. Bancroft in Australia, working quite independently, were not led astray. They were undeterred by statements that mosquitoes took one feed of blood, laid their eggs, and then died. They both found very soon that mosquitoes could be kept alive for astonishingly long periods (up to five months in one of Bancroft's later observations), and that they would suck blood repeatedly. Once this hurdle was surmounted, working out the developmental cycle of the parasites and indicating the correct method of infection were comparatively simple tasks. Manson showed that an insect was a host of a parasite of man; Ross and the younger Bancroft demonstrated that it was also an active infecting agent.

The century closed with insect-borne disease a firmly established and important phenomenon, and the new one began with the demonstration, outstanding for its courage, that not only were helminthic and protozoal infections transmitted in this way, but a dangerous virus too.

The Second Period.

The work just recorded set the tasks for the next twenty-five or thirty years. Substantial additions were made to the list of infections transmitted by Arthropods, while parasitology and medical entomology came into their own as major specialties; but, for the most part, the advance was general, a gathering in of knowledge lying ready to be picked up, the pursuit of logical consequences, rather than opening up of any entirely new territory. The explorers had found the country; the surveyors set out to map it. There were notable exceptions, as in the work on plague, on rickettsial diseases, and on flies as conveyors of intestinal infections, but the statement remains broadly true. The first World War and its aftermath especially influenced the last two and incidentally stimulated interest in medical entomology in Australia.

The contributions by Australians to a knowledge of transmission lay in two main directions—in plague and in dengue fever. Ashburton Thompson, in Sydney, in the years 1900 to 1903, was one of the first to observe the association between plague in man and epizootics in rats, and Tidswell in the same year recovered plague bacilli from fleas which had fed on infected rats. Australia was thus "in on the ground floor" in investigations of plague. Whether we can claim more is doubtful. The leader of the British Plague Commission of India, which completed the story, was C. J. Martin, who, like Fairley, was trained in Melbourne and did his early work there. Like Fairley, too, he returned to Australia to fill a need, when for a brief year or so he took over the Division of Animal Nutrition of the Council for Scientific and Industrial Research after the death of Brailsford Robertson. Nevertheless he spent most of his active life abroad, and his work on plague was Indian, not Australian. His demonstration with Bacot of the way the proventriculus of the rat flea becomes choked with plague bacilli and the dire results that follow therefrom was a little classic, which we would like to claim for an Australian, if not for Australia.

Australia played a more definite part in dengue research. Graham, in Syria in 1903, had pointed the way, but had used mixed batches of mosquitoes. T. L. Bancroft, in Brisbane in 1906, adduced shrewd epidemiological reasons to suspect *Aëdes ægypti*, and infected two volunteers experimentally, but his results were vitiated by the fact that he was working in an area where the disease was prevalent. It remained for Cleland, Bradley and MacDonald to put the issue beyond doubt in the 1916 epidemic, which extended well into New South Wales. They collected *Aëdes ægypti*, fed them on dengue patients at Mullumbimby and Grafton, transported them to Sydney, and there infected volunteers from whom they also passed the infection to other volunteers by blood inoculation. In the course of this work they not only established the method of transmission, but obtained evidence to suggest that *Culex fatigans* was not a good vector and added considerably to knowledge of the behaviour of the virus. Later, E. W. Ferguson (1928) published a thoughtful discussion of the epidemiology of the extensive outbreak that occurred in New South Wales in 1925-1926 and suggested that herd immunity, remaining effective longer than individual immunity, might account for the length of the interepidemic intervals.

Work on filariasis was mainly concerned with the search for possible intermediate hosts besides *Culex fatigans*, but T. L. Bancroft (1901, 1903) extended his original observations to *Dirofilaria immitis* of the dog, showed that it developed in "*Culex skusei*" (*Culex fatigans*), confirmed Grassi's finding that development took place in the Malpighian tubules (not the thoracic muscles, as in *Wuchereria bancrofti*) and, most important, demonstrated migration of the larvæ into the proboscis and their exit by rupture of the membrane at the base of the labella. Sweet, in 1924, confirmed Breinl's (1913) observations on nocturnal periodicity of the Queensland *Filaria*, and showed that it could be reversed by changing the sleeping habits of the patient. Mavis Walker, a Queenslander, also working with the Hookworm Campaign in 1924, found that *Wuchereria bancrofti* would develop at least to some extent in *Culex annulirostris*, but poorly, if at all, in *Aëdes vigilax* and *Aëdes ægypti*. Heydon (1931), on the other hand, got occasional development in *Aëdes vigilax*, but he found poor development in *Culex sitiens*, and confirmed the findings of previous observers that *Aëdes ægypti* was not a vector; he added both subspecies of *Anopheles amictus* to the list of good hosts. In 1934, really in our third period, Backhouse demonstrated that *Anopheles punctulatus* was an efficient vector in the Territory of New Guinea.

We must leave filariasis there, for, apart from recording some natural infection rates, for example in Anophelines in New Guinea during the recent war, Australians made no further contributions during the second or third period. That does not mean that there is nothing to be done. Many obscurities remain to be clarified, probably the most interesting being the status and distribution of the species in the Australasian region. There are at least two sibling species of *Wuchereria* infecting man in the region, with widely differing degrees of pathogenicity and, apparently, different insect vectors. Fortunately Australia seems only to have the least pathogenic, but non-periodic forms

occur patchily in New Guinea mixed with the widespread nocturnal type, and serious search has still to be made for *Wuchereria malayi*. Morphological and even serological differentiation seems to break down in this group of worms; perhaps chromosomal analysis (if the chromosomes can be found!) may yet throw a light on their taxonomy.

Of malaria one can say little. Abroad an understanding of epidemiology was growing apace; but Australia had little malaria, its hyperendemic territories in the islands to the north were far away, and so there was no stimulus to attack its unknown quantities. Breinl and Taylor (1918) studied an epidemic in Cairns, and various observers published records of individual sporadic cases contracted in the southern parts of Australia. There was what we know now to have been a rather exaggerated fear of malaria spreading in Australia after the first great war, so these records attracted serious attention. The one real achievement of the period was an admirable, clearly conceived and accurately recorded survey of Rabaul and certain other areas by G. A. M. Heydon in 1923, in the course of which he not only recorded precise data of spleen and parasite rates, but showed that the natural vectors in the area were the two subspecies of *Anopheles punctulatus*.

The Growth of Parasitology.

Parasitology was a study of quite respectable antiquity; it was respectable even before the time of Bancroft, Manson and Ross. The genus *Filaria* was described in 1787, *Gregarina* in 1828, *Trypanosoma* in 1843, the little hæmogregarine of the frog in 1850, to quote only a few examples. Nevertheless the discoveries of our first period gave it tremendous impetus. All parasites became interesting. Australians set to work to make known the parasitic fauna of this country, just as others were doing for their various parts of the world. Parasitologists held chairs in two Australian universities, the Australian Institute of Tropical Medicine during its brief career at Townsville did much parasitological work, as did the microbiological laboratory of the New South Wales Health Department, and Queensland had its Hookworm Campaign, which, as has been mentioned, did not restrict itself completely to that problem. As, however, we are concerned here only with the blood parasites and a few of their near relatives, it is convenient to review the contributions made by individuals rather than by institutions.

Native animals have been surveyed fairly widely for blood parasites, particularly by Johnston and Cleland, and a wealth of material awaits the enterprising experimentalist who seeks new strains for laboratory study. It is a point of interest that these species are much more abundant in the north than in the south, Sydney, in our experience, being about the southern limit of profitable search. The following incomplete list will give some idea of the extent and variety of this parasitic fauna.

Bats: A trypanosome and a *Plasmodium* described by Breinl (1913) in the flying fox (*Pteropus*). The *Plasmodium* is curious, and its life history is still obscure (Bearup and Lawrence, 1947), though Manwell (1946) described unpigmented extracellular schizonts.

Marsupials: Three species of hæmogregrarine (presumably *Hepatozoon*) described by Welsh, Dalyell and Barling; *"Filaria" websteri* Cobbold, in the knee joint of kangaroos; *Filaria trichosuri* Breinl in possums (*Trichosurus vulpecula*). This last is quite a promising experimental subject.

Monotremes: *Theileria tachyglossi*, found by Priestley in 1915 in the echidna, *Tachyglossus aculeatus*.

Birds: A great number and variety of parasites recorded including species of *Plasmodium*, *Hæmoproteus*, *Leucocytozoon*, *Trypanosoma* and *Filaria* (s.l.), the chief investigators being Gilruth, Sweet and Dodd (1910), Breinl (1913), Cleland and Johnston over a period of years, and Lawrence (1946). Curiously, as compared with the large numbers recorded from other parts of the world, only four bird hosts of *Plasmodium* are known in Australia, namely, *Biziura lobata* (musk duck), *Chenopsis atrata* (black swan), *Falco hypoleucus* (grey falcon), and *Passer domesticus* (domestic sparrow).

Reptiles: Species of *Hæmogregarina* (s.l.) from snakes, several species of lizard (very common, for example, in *Varanus*), and tortoises; trypanosomes from tortoises and geckos, *"Hæmocystidium"* (probably really *Hæmoproteus*) from tortoises; and *Filaria* from frilled lizards (*Clamydosaurus*) and jew lizards (*Amphibolurus barbatus*). T. L. Bancroft studied the species from the jew lizard, but did not publish his results. We have, however, slides he made showing the complete development of the worm in *Culex fatigans*. Geckos are a particularly rich source of parasites. One common species (*Gymnodactylus platurus*) living in the Hawkesbury sandstone around Sydney may be infected with no less than four species, a Trypanosome, a *"Hæmocystidium"*, a *Pirhæmocyton*, and a *Filaria*, sometimes all being present in the blood together. However, we could never find *Hæmogregarina* to complete the series, although this genus was well represented in other Geckonidæ from further north.

Frogs: Trypanosomes common in *Limnodynastes* species and *Lankesterella* in *Hyla cærulea*.

Fresh-water fishes: Trypanosomes, described from eels (*Anguilla* species) and cat-fish (*Tandanus*).

Marine fish: Trypanosomes, a *Trypanoplasma*, and hæmogregarines from several species.

Unless otherwise indicated, all the parasites of cold-blooded vertebrates were recorded by T. Harvey Johnston and J. B. Cleland in a series of papers from 1909 to 1916.

It is noteworthy that no species of *Leishmania* has been found in Australia, although T. L. Bancroft found the nearly related *Phytomonas* in plants, and *Herpetomonas* and *Crithidia* are known from insects. Bolliger, in Sydney, has nevertheless shown that the common possum (*Trichosurus vulpecula*) is a good experimental host for *Leishmania donovani*.

The routine use of splenectomy in parasite surveys will probably add many interesting species to the list of those we already know in native animals.

Mention may be made of the parasites of introduced and domestic animals to complete the story. *Dirofilaria immitis* in dogs has already been noted. T. L. Bancroft in Brisbane in 1888 was apparently the first to record *Trypanosoma lewisi* in rats in Australia, and *Hepatozoon muris* is also not uncommon. *Babesia bigemina*, the cause of red-water fever in cattle, was first seen in Queensland by Pound in 1895, *Theileria mutans* by Dodd in 1909, *Anaplasma marginale* by Legg in 1933, and *Babesia argentina* by the same worker in 1935 (already found by Filmer in Western Australia in 1931). Thus, Australia had acquired all the known types of tick-fever parasites, except the notorious *Theileria parva* and *Theileria annulata*. Moreover, Turner and Murnane (1930) found *Trypanosoma theileri* in cattle and *Trypanosoma melophagium* in sheep, and Mulhearn in 1946-1947 recorded *Spirochæta theileri*, *Bartonella bovis* and *Eperytherozoon bovis*, so the obscure species are here as well as the well known pathogens.

Considerable work has been done on immunity in these infections. Gilruth was responsible for the introduction of the benign *Anaplasma centrale* from Africa for use in immunization against the virulent *Anaplasma marginale*, while Legg obtained evidence that infection with *Babesia bigemina*, which can be controlled easily by specific therapy, confers some degree of cross immunity against the more dangerous *Babesia argentina*. In this way, a system of inoculations has been developed, which is used to control red-water fever in the north and especially to protect valuable stud stock brought from southern stations to enrich the northern herds.

Finally, there is the important group of closely related filarioid worms, which produce "worm-nodule" in cattle, and cause the loss of hundreds of thousands of pounds annually to the beef export trade. Cleland and Johnston in 1910 described the most frequent species (*Onchocerca gibsoni*), Breinl (1913) gave a detailed account of its morphology, and several workers (Cleland, Ferguson and Drabble, Johnston and M. J. Bancroft, and others)

attempted to find out how it was transmitted. That proved to be an exceedingly difficult problem, but a lead has been given by Blacklock's (1926) work on the transmission of *Onchocerca volvulus* by *Simulium damnosum* in Africa and Buckley's (1938) on transmission of *Onchocerca gibsoni* by *Culicoides*. Their observations are a challenge to parasitologists in Australia to determine the vectors here, especially as the advent of DDT raises some hopes that they may be controlled once they are known.

The Growth of Medical Entomology.

Like parasitology, the study of insects which are now of medical and veterinary importance is of respectable antiquity; but in the pre-Manson era it was simply part of general entomology with no special implications, whereas now it is practically a specialty on its own. The systematic entomologists, Weidemann, Macquart, Walker, Schiner and others, had described biting flies and other insects brought back to Europe by the early naturalists in the latter part of the eighteenth and first half of the nineteenth centuries, and the first investigations actually undertaken in Australia were also purely entomological. Skuse was a dipterist, and, from 1888 to 1890, he described a number of species of mosquitoes and sandflies (along with various other nematocerous diptera of no medical interest), and did it exceedingly well. His untimely death was a tragedy.

Later developments were a direct result of discoveries in insect transmission. Theobald set to work on his great monograph of the Culicidæ of the world; T. L. Bancroft sent him much material from Australia, and brought Theobald's descriptions, with his own field notes, together in a "List of the Mosquitoes of Queensland" in 1908. Then followed the establishment of the Institute at Townsville, where Taylor and, later, Hill added many species to the Australian list, and the latter contributed to our knowledge of their biology as well as of their taxonomy. Later again came Edwards of the British Museum, who, assisted largely by material sent to him by Hill and Ferguson revised the Australian species of mosquitoes, corrected Theobald's and Taylor's numerous mistakes, and set the systematics of the group on a firm foundation. This paper of Edwards in 1924 is the starting point of modern taxonomy of the group in this country. Other workers followed, of whom the most recent are Lee, Woodhill and Wharton in Sydney, and Miss E. N. Marks in Queensland. Miss Marks tells me that the list of known species and subspecies in the Australian region now totals 334, of which 120 occur in Queensland, a great increase on the 26 species known to Skuse.

In the meantime, the behaviour of mosquitoes was not neglected, though it did not receive as much attention as their systematics. Hill worked extensively in the field in Queensland, the Northern Territory and New Guinea, Fielding (1919) studied the bionomics of *Aëdes ægypti*, Cooling described additional life histories, and Hamlyn Harris pioneered ecological investigations in Queensland. Mackerras described the early stages of the then known Anophelines, and Woodhill initiated ecological studies of some important species in New South Wales, including the remarkable *Aëdes concolor* which normally breeds in highly saline rock pools along the coast. All this information was later of considerable value when the pressure of war came.

The sandflies were long neglected after the original studies of Skuse, now nearly sixty years old. Three families occur in Australia. The genus *Phlebotomus* (Psychodidæ), notorious in other continents as vectors of sandfly fever, leishmaniasis, and Carrion's disease, is represented in Australia by only three species and a variety, the first described by Hill in 1923 and the others by Tonnoir in 1935. American entomologists discovered additional species in North Queensland during the war, but none seems likely to be important in relation to disease. The undoubted scarcity in Australia of the group which attacks warm-blooded animals is a point to be borne in mind when obscure fevers are being studied. *"Sandfly fever" is a most improbable diagnosis in this country.*

The second family, the Ceratopogonidæ, comprises the minute pests of the mangroves (and of many inland places, too). They have no known relation to disease in man, though they may transmit one or other of the worm nodule parasites in cattle, and an investigation of their "venom" might be an interesting field for a student of the phenomenon of sensitization. Entomologically, too, they are practically a virgin field, which is only now being explored (*Lee, 1947*).

The third family is the Simuliidæ, also possible vectors of onchocerciasis. Again, there was a long gap from Skuse in 1889 to 1890 to Tonnoir in 1925. Only fifteen species have been described to date in Australia, but several more are known to occur. They are remarkable for the adaptations of their early stages to life in running water. The only serious pest in this family is a Queensland species, which causes great disturbance to man and beast in the more inland parts of the State. These intense waves of sandfly abundance follow with great regularity about a fortnight after flooding in the rivers, and last about a fortnight, unless there is further flooding. Very recently it has been found that the larvæ develop only in the fastest, most turbulent, muddiest parts of the strongly flowing, flooded rivers. They cling to the branches and fronds of submerged tea-trees (*Melaleuca*), gathering nourishment from the water that rushes past, and pupating in great crowds in the same situation. It used to be a mystery where the "Dawson River sandfly" came from, but a comparatively small amphibious field investigation removed at least a good part of the obscurity.

Work on the March flies (Tabanidæ) was greatly stimulated by the suspicion they were held in as possible vectors of onchocerciasis. Ferguson and Hill (together with the earlier work of White, Hardy and Taylor) put our knowledge of the family on a thoroughly firm foundation. The Ferguson collection, now housed in the School of Public Health and Tropical Medicine, Sydney, is a model of completeness and orderliness, and an indispensable tool to anyone who wishes to study the family in this country. Life history studes have been published by Johnston and M. J. Bancroft (1920), Hill (1921), and Mary Fuller (1936, 1937), but they have only touched the fringe of a huge and interesting subject.

There is one curious group of bloodsuckers in the peculiarly Australian genus *Spaniopsis*, which sits balanced somewhat precariously between the Tabanidæ and Leptidæ. These flies were studied by Ferguson (1915), and their interest here lies in the suggestion that has been made, though without real evidence, that they might be the cause of bung-eye.

The work on onchocerciasis shows how following "red herrings" is not waste effort in scientific work. It led to a sound knowledge of Tabanidæ, but, less obviously, it led to a fine contribution to general zoology in Miss Madge Henry's monograph of the fresh-water Entomostraca, which would probably never have been written if she had not been drawn to the group through her association with the early work on onchocerciasis.

On other groups, one must necessarily be very brief, but the foregoing will show how the general subject developed. Malloch's descriptions of Muscidæ and Calliphoridæ, largely on material sent to him by Ferguson, Hardy's contributions to the same families, Mary Fuller's life history studies and her fine work on carrion ecology, Harrison's work on lice and the use he made of the relationships of the parasites to elucidate obscure affinities in their hosts, and Womersley's studies of trombiculid mites, have all contributed to a balanced and well-documented knowledge of medical entomology in this country. As in every branch of science, there is still much to do, and perhaps one of the most urgent tasks is to build up a reference collection of ticks (and of fleas) which will be the equivalent of the Ferguson collection of Tabanidæ.

The Third Period.

Already, in tracing the development of particular lines of inquiry, we have invaded the third period at one point or another, and already some of its characteristics were apparent in the second period, so that one cannot draw any clear-cut distinction between the two. Its main trend, however, may be stated as a swing from the observational to the experimental method of approach. Descriptive work fell from favour; taxonomy was looked on as a dull and stodgy business, until the new taxonomy arose to replace the old; ecology came into its own; epidemiology (which is the ecology of disease) became a subject of the greatest importance; interest swung from dead-end results to the living interactions between hosts and parasites; studies of the physiology of parasites followed as a natural consequence; and everywhere the desire became apparent to build prevention and treatment on precise, detailed knowledge of behaviour, and to test results by accurate statistical means. A contributing factor, which has had a profound influence, has been the development by physicists and chemists of new tools of research, which have given some parts of biological work a precision and speed unattainable in the past (but which require, often, special skill to operate). With the rapid increase in knowledge and consequent growth of specialization (for the storage capacity of the ordinary human brain is limited!) came, too, the need to build teams, in which different points of view and different methods of approach can be welded together in a balanced attack on the complex problems that face the modern worker. Finally, there has been the influence of the last war, to which scientific research contributed, for good and ill, far more than in all the wars of the past.

This period is still but half as long as the others, yet it has already broken more new ground than the whole of the second period, and it gives promise of being the most fertile of all. There is a feeling in the air that we may be touching the unknown more closely than ever before.

Coming down to actual events, we have already touched on the developments in parasitology and medical entomology, so only a few pieces of work remain to be mentioned in the pre-war portion of this period. The first was a small but significant investigation, in which Bentley, Thompson and Williams (1929) showed for the first time that an Anopheline native to the mainland of Australia, namely *Anopheles annulipes*, could act as an efficient host of *Plasmodium vivax*. The second was the study of ephemeral fever in cattle undertaken by the Council for Scientific and Industrial Research, the third the recognition of scrub typhus in Australia and New Guinea, and the last, the most stimulating of them all, the discovery of a new rickettsial infection, "Q" fever, by Derrick and his associates in Brisbane. Brief notes may be offered on the last three.

Ephemeral fever wins a place in the story, not because the vector was recognized, for it was not, but because it provided a dramatic and relatively harmless demonstration of what happens when an infection invades a new country. The disease appeared first in the extreme north of the continent early in 1936, and it swept through the country like a forest fire, reaching the Victorian border in February, 1937. In its progress it jumped barriers, flared unexpectedly in places many miles from its line of march, and gave every indication that the spark of infection was air-borne, defying quarantine lines or restrictions of stock movement. The incidence of the disease was something over 90% in the affected population, whole herds were laid low almost simultaneously, and milk production in many areas was temporarily disorganized. Fortunately, as the name implies, the infection was transient and the mortality low. It vanished, like dengue, to which it is closely related, with the onset of cold weather, and now is only known as a patchy, smouldering endemic infection in various parts of Queensland.

The transmitting agent was not discovered, though sandflies (Ceratopogonidæ) were suspected in the end; but some interesting observations were made on the virus, among which was the demonstration that it was linked firmly to the leucocyte-platelet fraction of the blood. Antigens prepared by A. D. Campbell from this fraction gave good complement fixation, and a similar procedure might be useful in studying immunity in dengue fever. The filtration experiments, too, were a little unusual, for they involved team-work at a distance and the use of modern air transport, the suspensions being sent from Canberra to Dr. Burnet in Melbourne, who put them through Elford membranes, and returned the filtrates to Canberra for inoculation into clean cattle.

The story of the rickettsial infections in Australia has been told by Burnet in his Bancroft Lecture to this Branch in 1942. Hone's (1922) discovery of endemic typhus, and his suspicion that rodents were reservoirs, belongs chronologically to the second period though genetically here. Scrub typhus was clearly described by Breinl, Priestley and Fielding in 1914, and the combined labours of Langhan, Mathew, Unwin and Heaslip (1935-1941) separated it clearly from other obscure fevers of North Queensland and established its identity, while Heaslip's work incriminated *Trombicula deliensis* as the vector. Meanwhile, Gunther (1935-1940) was performing a similar service in New Guinea, so that, when war came to those areas, we knew something of what to expect, though few realized how serious the problem would become.

The work on "Q" fever, and the long range collaboration that developed between Derrick and his team in Brisbane and Burnet and his team in Melbourne was also described in Burnet's lecture. It began an association which we earnestly hope to foster and extend in the future. As an outsider, I think I am in a position detached enough to say that it represents precisely the sort of thing we want to do. It sought and found the cause, established a means to test for past or present infection, ferreted out the reservoirs and the vectors, not being content with one, but testing every accessible possibility; and it tested most of the links joining reservoir to man. Burnet believed that the final step was most probably by dust into the lungs, but this remains still for actual experimental demonstration, which, we hope, will not be too long delayed.

Wartime Research.

It is appropriate to introduce the wartime investigations by carrying the rickettsial story to its conclusion. Jungle fighting had scarcely begun when scrub typhus appeared, and the mortality rate rose alarmingly when the fighting was most arduous and the line of evacuation long and exhausting. It is correct to say that the soldiers feared typhus more than they did the Japs. But what was to be done? Cook endeavoured to define the dangerous situations, but that could not help much when the places to be occupied had to be dictated by tactics rather than typhus. At this time (end of 1942, beginning of 1943) dimethyl phthalate was proving valuable as a mosquito repellent in America and Australia, and McCulloch acted promptly on a report that it was also effective against "chiggers" in America. His first experiments were in safe areas in South Australia and New South Wales, but later he carried the work to the Atherton Tableland, and finally to New Guinea. He showed that dimethyl phthalate killed the mites, but that dibutyl phthalate was practically as effective and persisted very much longer; DDT, a marvel in other fields, was of little use here. He developed a technique for impregnating clothing by hand without any special equipment, which would have taken time to obtain, and then he turned himself into a travelling salesman, going round teaching units how to use the method, chasing failures in supply, and generally making a genial nuisance of himself until he had the whole procedure organized into a smoothly running routine. The graph in his published paper (1946) is a striking demonstration of the results that were obtained, and they were confirmed later by the general adoption of his method by British forces in tropical areas.

McCulloch's work was magnificent, and so was the cooperation of those troops who submitted themselves voluntarily, without tangible reward, to the risks of infection in the whole series of New Guinea experiments.

We must pass over the clinical and pathological studies of Williams, Sinclair and Jackson (1944), McCulloch's (1944) work on the field biology of the mites, and Womersley's (1943, 1944) on their taxonomy, all of which helped to a better understanding; but it would be unjust to the members of the United States Typhus Commission working in New Guinea not to refer to their detailed experimental investigations. They stuck to dimethyl phthalate for control, wherein we did not agree with them, but they isolated the Rickettsia, and gave concrete proof that *Trombicula fletcheri* and *Trombicula walchi* (? = *deliensis*) were vectors and *Rattus concolor browni* a reservoir, bringing clear light where previously there was suspicion but no certainty.

Before I come to the malaria investigations, which naturally occupied so much of the field, a word may be said of a small piece of dengue research which was not without its own special interest. We were all too familiar with the ordinary urban dengue transmitted by *Aëdes ægypti*, but outbreaks began to occur in New Guinea in areas far removed from former settlement, a condition rather reminiscent of jungle yellow fever. Obviously some other vector was at work. We followed the technique established by Cleland and his colleagues nearly twenty years earlier. After a few unsuccessful attempts had been made with other species, Atherton bred some *Aëdes scutellaris*, fed them on blood from patients with dengue, and flew them from New Guinea to Sydney (there was no difficulty by then in obtaining a priority for such an important cargo as a batch of experimental mosquitoes). There they were nursed by Woodhill and Lee, and fed on volunteers, who duly contracted the infection, as was proved by subsequent passage to other volunteers. Specific control of *Aëdes scutellaris* was the logical sequel, not always easy in practice, but not without its results.

Finally, we come to malaria research, a subject which merits a lecture to itself. Fortunately, you have already had that lecture from Brigadier Fairley (Bancroft Lecture, 1945), and many of the results have already been published or are in course of publication. It is not always appreciated, however, that the investigations were in four distinct but interrelated directions, and that each contributed to the results achieved.

We may begin appropriately with Heydon. In 1942, he investigated the sharp outbreak of benign tertian malaria which was causing anxiety at Cairns. He showed that the vector was a particular Anopheline, which was identified first by Woodhill and later by Taylor as *Anopheles punctulatus moluccensis*. That was of value in local control, but it went further, because it was a liberal education to the army entomologists who were associated with him. These men went to New Guinea, and, in the midst of arduous control work, still found time to study sporozoite rates and collect material for analysis of blood feeds. Studies of spleen and parasite rates in natives and troops (and finally, after the capitulation, in Japanese) went on *pari passu*, and the whole combined to form a reasonably adequate picture of the sources of infection and the local epidemiology of the disease.

The second phase was purely entomological. We had to know precisely with what insects we were dealing, and we had to learn a great deal about their habits and behaviour—and this, as will be evident from the discussion of dengue, applied to Culicines as well as Anophelines. Lee in the University of Sydney (for we had our "backroom boys" just as had more exalted circles) and Woodhill, partly in the field and partly at the university, concentrated on the first aspect of the problem. They cleared away many obscurities and published several papers, of which Lee's "Atlas of the Culicine Larvæ", published by the army, and their fine joint "Monograph of the Anophelines", published by the university, were the most outstanding. Meantime, Allman, Atherton, Roberts, and many others were at work on biological studies in the field, so that we really began to know our enemies.[1] A few of their results have already been published, some are in course of publication, and others are still in preparation. It would be a great pity if preoccupation with post-war distractions caused any of this work to be lost to the world.

The third phase was direct control, a big subject, but we are concerned here only with its investigation, not its application. The "back-room boys" in this instance were Waterhouse and his associates in the Council for Scientific and Industrial Research at Canberra. They worked out standards for malaria oil in the early days and for pyrethrum sprays to kill adult mosquitoes, and they provided, too, some basic data for the application of DDT. Most important, they gave us dimethyl phthalate as a repellent, working parallel with the Americans in Florida, and so quickly that the Australian forces were using this preparation as a routine in the field many months before either the Americans or the British. Waterhouse spent his time half out of the army and half in it, so that he could carry his laboratory findings immediately into field testing in association with the full-time army entomologists.

The development of DDT revolutionized control work. American and British results were quickly adapted to New Guinea needs, so that the demand for long ran ahead of the supply. Malcolm and Conroy developed the simple dropper-bottle method of application at Moresby, and Simmonds proved its worth in the operations on the north coast, bringing the newly occupied area under control with a promptitude which would previously have been unimaginable. The use of aircraft to distribute DDT was naturally slower to develop, for it was a complicated business. Ratcliffe, Gilmour (Royal Australian Air Force) and Mules were the men most responsible. Australia diverged from other countries in using a much finer spray, and in taking special advantage of natural down-draughts of air to bring it to the ground. The results were extremely good.

As a result of all this work, it was shown that an epidemic could be broken (the graph of malaria incidence at Milne Bay is a dramatic example), and that a general incidence of about five cases per thousand per week could be maintained by mosquito control alone, for the suppressive drug regimen of those days was subsequently proved to be inadequate. This, however, applied only to back areas. We failed to protect troops in contact with the enemy, although there is later evidence that combined control from ground and air did reduce the amount of malaria in a semi-forward area at Wewak, where a relatively "Atebrin"-resistant strain of malignant tertian parasite was encountered.

It was here that the Cairns unit reaped its great harvest. A fairly adequate régime of suppression had been developed slowly and cautiously, for no one in those days knew what dire results might follow continued high dosage of "Atebrin". But few people outside the medical services really believed in it; rigid proof had to be produced and rammed home. Cairns, under the guidance of Brigadier Fairley and the leadership of Lieutenant-Colonel Blackburn, became the greatest mass-production centre of experimental infections the world has seen. It produced the proof promptly and in abundance, Brigadier Fairley rammed it home, and malaria ceased to be a menace even to the most forward and isolated troops. Five cases per thousand troops at risk per week were considered a satisfactory result for mosquito control in back areas under the prevailing conditions; the taking of suppressive "Atebrin", adequately disciplined, kept the rates uniformly below one per thousand per week in all areas, forward or rear, with the single exception of Wewak. Mosquito control had the virtue that it left no hidden residue of relapsing benign tertian malaria, but the operational advantages of "Atebrin" were enormous.

[1] It is not to be forgotten that Heydon (1923) twenty years earlier gave as terse and clear an account of the essential bionomics of *Anopheles punctulatus* as has ever been published.

The work at Cairns did not stop there. It provided a critical analysis of other newer suppressive drugs, it demonstrated the special worth of "Paludrine", and it accumulated a mass of information on the natural history and behaviour of malarial infection, the parasites and the vectors. It is not inappropriate that a descendant of Joseph Bancroft should have been responsible for the mass production of infections which Fairley and Blackburn used to such advantage.

The losses of war are great and the gains are few. At least we can count those scientific results which I have attempted to outline so briefly tonight among the permanent gains, for they can result in nothing but good.

Conclusion.

We have travelled a long way in the seventy years since Bancroft first fished out a female filarial worm on his "peculiar trochar", and we have learned a great deal, not only of phenomena and processes, of causes and effects, but of methods, of points of view, and of the spirit of research. Much of the knowledge acquired has been of immediate practical utility, much of it has not, but who can say which will prove of greater ultimate value to human progress? Again and again the way to prevention of disease pointed by discoveries in this field has proved not to be as broad and clear as it seemed at first sight. All sorts of impractical, academic studies have had to contribute before control could be made efficient. Even during the war basic biological research was found to be indispensable, as perhaps our American colleagues appreciated better than we did. Surely, now that some sort of peace has come, we can hope for leisure and freedom to persue the fascinating lines of inquiry that lie before us, thinking first of understanding, and but secondly of utility.

The greatest asset of the research worker is curiosity; his motto, like that of Rikki-tikki, is "run and find out". He does not want much money, but he does want mental elbow-room, a stimulating atmosphere, and all sorts of tools and gadgets to do his tricks with. We are embarking on a new venture here in Queensland, still in the third period of research, and there is an abundance of tasks before us, not only in insect transmission and the behaviour of parasites, but in other fields of infectious disease as well. May we not hope that our new institute will remember those of whom I have spoken tonight, and come in time to be recognized as a place where the spirit of research always dwells?

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GASTRIC SURGERY WITH SPECIAL REFERENCE TO PEPTIC ULCER.[1]

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THIS short paper is intended to be a general discussion on the indications for gastric surgery and on the suspected merits and demerits of the numerous procedures advocated by different writers and workers. I think the best way to approach the subject is briefly to outline the physiology of the stomach and then to deal with the main pathological conditions under separate headings. I should mention straight away that the medical treatment of ulcers and some other conditions should always be adequately tried before resort is had to surgery. In a large series, namely, 732 patients in one hospital, satisfactory results were obtained by medical treatment in 55%; 35% were later subjected to operation as they had not been relieved; the results were unknown or not recorded in 10% of cases. The writers point out that these results will become more unsatisfactory as the duration of the follow-up period increases. Twenty-two deaths (3·5%) took place during the follow-up period and were directly due to the ulcer. Other figures are quite different, some showing a much higher percentage of good results from medical treatment. The results undoubtedly depend to some extent on the type of patient and on the social and economic conditions *et cetera*.

It has been recognized for many years that the primary gastric secretion or appetite juice is nervous in origin and is brought about by the sight or taste of food. It has also been known for a long time that about half an hour after food enters the stomach a much greater flow of juice takes place. Many workers advanced various theories as to the nature and origin of the exciting substance, but it is only fairly recently that it has been proved that certain food substances cause the mucosa of the pyloric region of the stomach to produce a hormone; this has been called "gastrin". It is thought that this passes into the portal blood-stream and later passes via the systemic circulation into the wall of the body of the stomach and here stimulates the oxyntic cells of the mucosa, and thus further hydrochloric acid is produced. It should be mentioned that there are no acid-producing cells in the pylorus or in the cardiac end of the stomach; they occur in the body of the stomach and cease about four to six centimetres from the pyloric outlet and the same distance from the œsophagus. When the gastric contents leave the stomach, probably a further secretion of hydrochloric acid is brought about by an intestinal hormone; this is thought to be produced particularly by the duodenum and is passed via the blood-stream to the stomach cells; this latter substance has been called "enterogastrone". From the standpoint of gastric surgery it is the pyloric hormone which is important, for although the cause of peptic ulceration is not definitely known, it is certain that high hydrochloric acid values are present in most duodenal ulcer patients and in a fair proportion of gastric ulcer patients, and further, it is a fact that reduction of hydrochloric acid secretion does help healing and does relieve symptoms. It will therefore be seen that all operations should be planned with this in mind. There are thus two ways by which the amount of hydrochloric acid can be reduced surgically: (i) excision of the pylorus resulting in no further production of gastrin, and (ii) resection of a proportion of acid-producing cells of the stomach.

When gastro-enterostomies were performed for all types of ulcers, and often when no ulcer was present, the object was to reduce acidity by permitting small intestine contents, which are alkaline, to pass into the stomach and thus neutralize the hydrochloric acid. This did not work out in practice, and we all know of many bad results. Further, since the days of mass gastro-enterostomies it has been shown that the decline in hydrochloric acid some hours after a meal is due mainly to an increased flow of gastric juice from the pyloric mucosa, this being alkaline and rich in mucus; the reduction in acid value at this period is due only in a small degree to regurgitation of alkaline small intestinal contents. It is thus clear that the very theory on which many gastro-enterostomies were planned was wrong. It may be thought that this alkaline pyloric juice is desirable in the treatment of peptic ulcer, and that therefore the statement made earlier about resection of the pylorus is a contradiction; but I think the answer is that the amount of acid produced as a result of the substance gastrin is out of all proportion to what may be neutralized by the alkaline juice. In any case, as will be shown later, most of the modern radical procedures for the treatment of ulcers endeavour to combine the two aspects—namely, excision of all or most of the pylorus and resection of a considerable portion of the acid-producing part of the stomach.

TABLE I.

Operative Procedure.	Gastric Acidity.		
	Before Operation.	Three Weeks After Operation.	One to Five Years After Operation.
Ligature of arteries *plus* partial gastro-enterostomy (82 cases)	65·3°	45·0°	41·0°
Ligature of arteries only (14 cases)	62·0°	39·0°	44·0°

It is perhaps appropriate to refer to one other operation which aims at a reduction of acidity by other means—

[1] Read at a meeting of the Lincolnshire Branch of the British Medical Association in June, 1947.

namely, physiological gastrectomy. This consists in the ligating of three-quarters to five-sixths of the arteries of the stomach. It has been performed with or without a gastro-enterostomy; in the latter case it is held to prevent anastomotic ulcer. Somervell, a surgeon in India, claims uniformly good results, and puts this procedure forward as the answer to the vexed question of what is the correct operation in cases of duodenal ulcer associated with high acid curves and failure of response to medical treatment.

If these results are reliable, and if the numbers are large enough to be significant, then it does seem that the procedure is justifiable. This particular surgeon states there have been only one or two cases in which the amount of hydrochloric acid failed to drop appreciably and in which the results have not been excellent. He attributes these bad results to failure to ligate a sufficient number of gastric blood vessels. He further refers to one case in which the operation was performed by another surgeon and in which death resulted from gangrene of the spleen. I have had one disaster; in this case the patient appeared to die of ileus with or without acute dilatation of the stomach. The cause of death was not confirmed, as a post-mortem examination was refused. If the cause of death in my own case is correct, then it makes one wonder whether there is not too much interference with the nerve supply through the blood vessels, and whether the cutting off of the blood supply can *ipso facto* produce dilatation. Since beginning the preparation of this paper I have spoken to several other surgeons who failed to reproduce Somervell's results, so it seems that the method has not yet been proved to be a good form of treatment.

More recently still papers are appearing advocating vagotomy as a treatment for peptic ulceration. Some of the writers are very enthusiastic about the results; but I think it is far too early to judge. A recent investigation by workers in the Mayo Clinic suggests that the results are uncertain, and they are of the opinion that no conclusions should yet be drawn.

I shall now attempt to deal with the main pathological conditions which affect the stomach and duodenum and with the common surgical procedures which are carried out to alleviate them.

I do not propose to discuss some of the rarer conditions of the stomach, such as the several forms of tuberculosis and syphilis and the various forms of simple tumour, all types of which are rare. Sarcoma may occur in one of several forms, but is also a great rarity. In passing, one may mention that lymphadenoma occasionally first manifests itself as an infiltration of the wall of the stomach.

Most authorities are agreed that operation is the treatment of choice for an acute perforated ulcer, but our ideas have recently been somewhat shattered by the suggestion that acute perforation should be treated by conservative methods. You will have seen in the journals in the last few months an account of this method. Briefly, it amounts to the passage of a Ryle's tube and continuous suction, together with intravenous and sulphonamide therapy. One surgeon treated fourteen patients with three deaths; this, to my mind, is certainly a higher mortality rate than that which most of us obtain by immediate operation supplemented by heavy sulphonamide dosage. The intravenous administration of fluids is, of course, an essential part of the latter treatment.

Hæmorrhage.

I think it is conceded by all writers that a majority of patients with a bleeding ulcer recover with conservative treatment; but it is a fact that patients with a massive hæmorrhage often die. In one series of cases classified as of serious hæmorrhage there was a mortality rate of 13%. These patients were treated by medical or surgical methods. In some clinics the mortality rate is reported as being as high as 25% or even 30%. These figures are certainly far higher than our own in Scunthorpe.

When one studies these patients, either in the operating theatre or on the post-mortem table, one finds the reason why the hæmorrhage is difficult to control. It is the location of the ulcer, the unyielding fibrous tissue, and—most important of all—the size of the eroded vessel. In the case of hæmorrhage from a duodenal ulcer, it is usually found that the superior pancreatico-duodenal artery or one of its larger branches has been eroded. One can usually pass a matchstick or a large probe into the artery from the base of the ulcer. It can thus be readily realized how torrential the hæmorrhage may be. The rigid fibrous tissue prevents retraction or contraction of the artery. In the case of hæmorrhage from the stomach, the ulcer is frequently on the lesser curvature and has usually led to an erosion of the left or right gastric artery or of a major branch. In the majority of such cases the ulcer is chronic; but according to some writers as many as 25% of cases of massive hæmorrhage are due to erosion of an artery by an acute ulcer.

Recently I was asked by my physician colleague to examine a patient with massive and repeated hæmorrhage. The patient's condition was never good enough to permit of investigation, so the location of the ulcer was not known. After he had received seven pints of blood the abdomen was opened, a continuous drip blood transfusion being in operation, and a search was made for the ulcer. No abnormality was demonstrated on viewing or palpation of the stomach and duodenum; the liver and spleen were normal, so it was decided to open the stomach and duodenum and inspect the mucosa. This was done, and no abnormality whatever was found. Any area that appeared suspect was gently rubbed with a finger or moist gauze, but no hæmorrhage took place and no induration could be demonstrated. At the end of the operation the lower end of the œsophagus was inspected through an œsophagoscope, but no varix could be seen. Twenty-four hours after operation it again became evident that hæmorrhage had recurred; and in spite of blood transfusions the patient died. At autopsy this man was found to have a very small ulcer with practically no induration; but when a probe was passed into the main trunk of the superior pancreatico-duodenal artery it emerged in the duodenum, demonstrating that the arterial wall had been eroded by this very small acute ulcer.

It is my opinion that the acute ulcer does not, fortunately, often lead to massive hæmorrhage; but if it does so, then the medical and surgical problem is difficult. In the case of a chronic ulcer causing hæmorrhage, it is probably better to operate if the hæmorrhage occurs on more than two occasions and if it is fairly serious. But if a very serious and dangerous hæmorrhage occurs even one one occasion, it is, I think, advisable to give the patient liberal blood transfusions and operate as soon as the blood volume has reached a reasonable level. There has recently been a controversy in THE MEDICAL JOURNAL OF AUSTRALIA on the type of blood which should be transfused. On one side it has been argued that uncitrated blood should be used, as this will and does lead to clotting in the eroded vessel, while citrated blood does not so help. The other school suggests that the presence of citrate makes no difference. On theoretical grounds the absence of citrate should be better; but there are technical difficulties in a direct transfusion, and the Kimpton tube method requires a very experienced houseman.

One point should be mentioned here in the conservative treatment of severe hæmorrhage, and that is that morphine should be used with care. It has been pointed out by Finsterer and others that morphine may have a detrimental effect on a respiratory centre which is already depressed owing to the anæmia.

The medical treatment of hæmorrhage is divided into two main methods: (i) the starvation and rest method; (ii) the Meulengracht method. This latter method, briefly, permits of a full diet from the time the hæmorrhage starts, at the same time as alkalis, extract of hyoscyamus and iron are given. The argument in favour of this method is that a full stomach is more at rest than an empty stomach which is undergoing hunger contractions, and further, that the patient's already depleted strength is not diminished further.

I think one should sum up the treatment of serious hæmorrhage by stating that medical treatment should at first be tried at the same time as a blood transfusion is given; if it becomes obvious that the hæmorrhage is continuing, as evidenced by the vomitus or by the amount of melena, then surgical measures should be undertaken.

Finsterer, for about fifteen years, has advocated operation in all these cases as an emergency measure, the operation consisting of a partial gastrectomy. He claimed a mortality rate of under 10%, which, he pointed out, was lower than the mortality rate in cases of hæmorrhage treated by more conservative measures. This was probably true up to a few years ago; but with modern blood banks I think the mortality rate must now be less than 10% when these patients are treated conservatively.

With regard to the value of having blood "on tap", I recently performed a partial gastrectomy for recurrent hæmatemesis, and on the eighth or tenth day a very severe hæmorrhage took place, presumably from the suture line; it was necessary to give this patient twelve pints of blood over a period of a few days before his hæmorrhage stopped. I think one must certainly say that such a patient would have died before we had the blood banks. A further case will indicate the value of the blood banks, and also it will serve to emphasize that operation is essential in certain cases.

An airman, aged twenty-two years, was admitted to hospital with a mild hæmatemesis; twenty-four hours later a massive hæmorrhage occurred, so that four pints of blood had to be transfused at a maximal rate and then two continuous drip transfusions were left in operation. The melena and hæmatemesis still continued at an alarming rate, so a surgical opinion was sought. I advised operation, and the patient was placed on the table with an uncountable pulse. At this time he was receiving the tenth pint of blood, which was running in under pressure. A partial gastrectomy was performed and the patient finished the operation with a pulse rate of 84 per minute and was then having his fourteenth pint of blood. The hæmorrhage was due to the erosion of a large artery in the head of the pancreas. The ulcer was duodenal.

The actual operation performed may be a partial gastrectomy, or it may be a local excision of the ulcer and suture of the stomach.

All my references to hæmorrhage have been in relation to peptic ulcer, for I believe that severe hæmorrhage and certainly torrential hæmorrhages are rare in carcinoma or other growths of the stomach or duodenum. Large hæmorrhages can of course occur in the case of varicosities in the lower part of the œsophagus, but this condition does not come within the scope of this paper. One other disease of the stomach does occasionally cause serious hæmorrhage, and this is chronic hypertrophic gastritis. As far as I know there is no exact understanding of the pathology of this condition, and the treatment appears to be medical and even then very uncertain in its results.

Treatment of Uncomplicated Gastric Ulcer.

The medical treatment of uncomplicated gastric ulcer should be persisted with for at least six months; if there is then no radiological and clinical evidence of improvement, operation if practicable should be carried out. If a large crater is present, then some surgeons advocate earlier operation, especially if penetration into the pancreas has occurred. Partial gastrectomy is the operation of choice, as it removes the ulcer-bearing area and reduces the amount of hydrochloric acid. If the ulcer is small and on the anterior wall of the stomach local excision may be sufficient; most writers suggest that a posterior gastro-enterostomy should be performed at the same time, but this I think is open to question. For ulcers on the posterior wall or on the lesser curvature the same treatment is advised; but all seem to agree that for large ulcers partial gastrectomy is necessary. The great surgical problem is the large ulcer on the lesser curvature near the cardia; an abdominal gastrectomy is a difficult procedure in such a case, but with improved anæsthesia—for example, subcostal block with curare and "Pentothal" or light cyclopropane anæsthesia—an adequate gastrectomy can usually be performed. Some surgeons advocate a partial gastrectomy below the ulcer to do away with Edkin's hormone. This hardly seems a sound procedure, but in any case I have had no personal experience of the operation. Some years ago when we were not blessed with specialist anæsthetists I performed a high anterior gastro-enterostomy, as advocated by Wilkie and others, in seven or eight cases of large ulcers high up on the lesser curvature. In every case the results were first class, and some of the patients have been followed for ten years. It is difficult to know why the results should be so good, but one must remember that the number is very small and so no deductions should be drawn.

With improved anæsthesia giving greater relaxation one can often perform a two-thirds to five-sixths gastrectomy in suitable subjects. This brings a fair proportion of the lesser curvature ulcers into the class which can be adequately resected by the abdominal route. These lesser curvature ulcers often appear on the X-ray plates as only medium-sized craters; but when the stomach is examined the ulcer mass may be found to be 10 or even 12 centimetres in diameter, and the stomach wall in this area may be two centimetres thick. It will be appreciated that such a finding makes a partial gastrectomy very difficult and dangerous, and may render an adequate operation impossible in certain cases.

In the case of pyloric stenosis resulting from a gastric ulcer, there is no doubt that the best treatment is gastro-enterostomy if one is sure that the ulcer is simple in nature. The operation should in general be of the posterior variety with a medium-sized stoma. Sometimes it is technically difficult or impossible to perform the posterior operation, in which case an anterior gastro-enterostomy has to be done. Further reference will be made to pyloric stenosis and its treatment when the question of the incidence of malignancy in gastric ulcer is discussed.

For hourglass deformity of the stomach a number of operations have been advocated. A so-called sleeve resection is preferred by some, others prefer a gastro-gastrostomy, and still others a double anastomosis between the small intestine and the two sacs of the stomach. However, probably the best procedure is a partial gastrectomy if this is technically feasible.

Duodenal Ulcer.

Chronic duodenal ulcers almost certainly show a greater tendency to heal under medical treatment than chronic gastric ulcers. Conservative measures should be persisted with for at least two years, and there is a group of medical men who hold that surgery has no place in the treatment of duodenal ulcer. The generally accepted indications for operation in this type of ulcer are the following: no clinical or radiological evidence of healing after two years of strict medical treatment; frequent relapses after careful medical treatment; and, of course, the indications which have already been mentioned—for example, stenosis, repeated or massive hæmorrhage and perforation. The operative treatment that used to be suggested was pyloroplasty with local excision of the ulcer, but this often leads to secondary ulceration. Gastro-duodenostomy was favoured for a long time, but in practice it has been generally unsuccessful; further, the operation is difficult and the resulting œdema of the duodenum often leads to serious post-operative complications. Partial gastrectomy is therefore becoming increasingly popular. The resection should be adequate, all the pyloric antrum and as much of the body of the stomach as possible being removed, particularly the lesser curvature part. However, gastro-jejunal ulceration is not by any means unknown after this procedure.

TABLE II.

Author.	Inoperable Duodenal Ulcers. (Percentage.)
Mandl	58
Jacobovici	50
Koenig	14
Zukschwerdt and Eck	19
Berger	8

In dealing with the surgery of duodenal ulcers various authors have referred to the percentage of non-resectable or inoperable duodenal ulcers. However, there is great variation in this incidence according to the interpretation of the phrase and probably according to the skill of the surgeon. Here are some figures. (Table II.)

The following figures (Table III) will show that as a surgeon's experience increases, so the percentage of so-called inoperable duodenal ulcers decreases.

There are a number of reasons why an ulcer may be considered non-resectable, and these reasons will vary with different surgeons: but some of the important ones are: (i) involvement of the papilla of Vater, or its proximity in the ulcerated area, (ii) extensive penetration in the region of the common bile duct, (iii) penetration into the gall-bladder. The first reason is probably very rare; the

TABLE III.

Author.	First Report.	Later Report.
Finsterer ..	16·6%	6·0%
Flörcken ..	20·0%	10·0%
Ruge	80·0%	3·3%

third reason should rarely, if ever, preclude resection, as the gall-bladder should be removed at the same operation. On the other hand, Maingot advises the von Rothenberg occlusion operation in such cases, and holds that the resulting rest to the duodenal ulcer always leads to its cure.

Carcinoma of the Stomach.

As we all know, carcinoma of the stomach is common, whereas carcinoma of the duodenum is rare. There are four or five types of carcinoma of the stomach, but their differences do not really concern us in this talk. On one occasion I heard Balfour of the Mayo Clinic give a lecture on this subject, and he had a very depressing story to tell: of some hundreds of patients with gastric carcinoma who came to his clinic, 50% obviously were in a clinically inoperable condition owing to the presence of secondary deposits; of the remaining 50%, half were opened and closed up again without any attempt at a curative or palliative operation—again, of course, owing to the presence of secondary deposits in the liver *et cetera*; in half of the remaining 25% of cases a radical operation was performed in an attempt to cure the patient, and a palliative operation was performed in the remainder. Balfour ended by stating that the number of patients alive three and five years after the operation was small.

The treatment of patients in an operable condition is of course gastrectomy, either partial or total. In cases in which the pylorus or the lower half or two-thirds of the stomach is involved, the operation can be performed satisfactorily through an adequate abdominal incision when the necessary area of stomach is resected, and an anastomosis made between the upper part of the stomach and the duodenum or jejunum. Good anæsthesia, preferably local anæsthesia with light cyclopropane or "Pentothal" anæsthesia, is essential. In the case of growths involving the upper half of the stomach, I am convinced that the approach should be thoraco-abdominal. In this operation the gastro-hepatic omentum is divided well away from the stomach, and the whole of the stomach is removed together with the body of the pancreas and associated glands, the spleen and the greater omentum. It is important that the glands in relation to the left gastric artery should also be removed. In some cases the lower two inches of œsophagus are removed with the supradiaphragmatic lymph glands. The duodenum is carefully closed off and then an anastomosis is made between the lower end of the œsophagus and a suitable part of the upper end of the jejunum. In passing I should like to point out that a similar operation—namely, œsophago-gastrostomy—can be applied in the case of growths of the lower two-thirds of the œsophagus. The stomach when adequately freed can easily be pulled up as far as the aortic arch. The operation of total gastrectomy for carcinoma of the body and fundus of the stomach performed through an abdominal incision has to my mind been superseded by the above-mentioned procedure, and I am sure that the risks of the single abdominal procedure, owing to the difficulties of anastomosing the jejunum or duodenum to the œsophagus, are greater than those of opening the chest cavity in order to make the anastomosis. With our present knowledge, however, the outlook will be gloomy for carcinoma of the stomach. As far as I am concerned I have three patients alive for more than two years after radical resection for carcinoma of the stomach. The one who has lived longest is a woman, aged thirty-seven years, who had an ulcerative form of growth in the lower half of the stomach and was subjected to partial gastrectomy in October, 1944. I have used the thoraco-abdominal approach in six cases of carcinoma of the body of the stomach or of the lower part of the œsophagus, and can say that the operation, though tedious, is far from difficult. The same approach was used in a case of gastro-jejunal-colic fistula, and though the operation was completed in one stage there were no complications from infection.

Comparison of Results of Gastro-Enterostomy and Gastric Resection in the Treatment of Peptic Ulcer.

One American writer has analysed the results of gastro-enterostomy and gastric resection in the treatment of peptic ulcer for periods of up to ten years. He admits that the trend has been towards resection since 1939. His opinion is that, with the lapse of time, a patient with a gastro-enterostomy tends to have a recurrence of symptoms, but there is also some tendency towards this after gastric resection, although this is not so pronounced. His mortality figures for the operation are 7·7% for resection and 5·9% for gastro-enterostomy; both figures, to my mind, are high, especially that for gastro-enterostomy. In this particular section of the paper there is no reference to the individual acid curves. The question of gastric secretion is discussed later in the paper, and he points out that of 75 patients after gastro-enterostomy, 92% were found to have an amount of free hydrochloric acid equal to, and in the large majority of instances greater than, the normal, while only 8% had figures below normal. A further chart compares the pre-operative and post-operative gastric acidity of 50 patients who had undergone a gastro-enterostomy operation; in 28 patients the pre-operative acidity was higher than the post-operative acidity. However, this authority points out that he can find no correlation between the clinical results and the acidity. In other words, some patients with a sustained high acid curve after operation get good results, and in others with unsatisfactory results the hydrochloric acid value was high or low.

In the case of gastric resection, among duodenal ulcer patients the following post-operative figures were found: no acid in 27%, a low acid figure (that is, below normal) in 24%, and no significant change in 49%. In the case of gastric ulcer the effect of resection on the acid curve was as follows: achlorhydria in 72%, hypochlorhydria in 20%, and no significant change in 8%.

In the summing up of these last-mentioned figures, it may be said that there is some evidence that gastric resection reduced acid secretion, and this reduction was probably in direct proportion to the extent of the resection, and further, results were probably better for those with a low post-operative acid curve. After gastro-enterostomy, as has already been mentioned, there is no significant change in acid secretion; yet 75% of the patients obtained satisfactory results.

It is difficult to explain why the percentage of good results should be as high as 75, but I think the explanation may be that a number of these ulcers would heal spontaneously with or without full dietary treatment, and further, the operation would undoubtedly be performed in a certain percentage of cases whether there was or was not a true indication according to recent concepts.

After gastric resection there were 90% of good results, while a large percentage of patients had a reduction in acid secretion.

A further comment by this American author is that as large resections tend to increase the mortality rate and yet tend to give little better results, then an operation of lesser magnitude—that is, a smaller resection—is probably desirable. However, I think that these results do not help very much in assessing the value of any particular pro-

cedure unless it is analysed in terms of the position of the ulcer. When that is done, it is found that an adequate resection for gastric ulcer gives a very high percentage of good results. In the case of duodenal ulcer, in which the acid curve is usually much higher than in gastric ulcer, the results are not so good, mainly, I think, owing to the occurrence of anastomotic ulcer. I am convinced that in duodenal ulcer cases, if resection is carried out at all, the amount of stomach removed must be large, its upper limit on the lesser curvature extending almost to the œsophagus.

Gastro-Jejunal Ulceration.

Gastro-jejunal ulceration is a serious complication of a gastro-enterostomy or of an anastomosis between the stomach and jejunum after gastric resection. The incidence varies greatly in different clinics and according to the variation in the types of operation performed and also because gastro-enterostomies are performed often without real indication. The various figures which have been published suggest that the incidence of this form of ulcer varies between 4% and 15%. It is, of course, often difficult to be sure whether the patient has developed his ulcer symptoms from the old ulcer for which the operation was performed or whether the symptoms are due to an actual anastomotic ulcer.

The incidence of jejunal ulceration after gastro-enterostomy or after palliative resection is put by another writer at a figure of 12% to 30%, whereas he states that the incidence after the more radical procedure of adequate partial gastrectomy does not exceed 2% to 5%.

It is generally conceded that marginal ulcer is one of the most serious complications that can develop after an operation on the stomach; in fact, the value of a good surgical procedure should be measured by the frequency with which marginal ulcer follows it. Balfour, in 1927, reported an incidence of 2% of jejunal ulcer after gastro-enterostomy. I find it very difficult to believe that this figure is correct, for at that time gastro-enterostomies were very fashionable and were performed for almost any form of peptic ulcer. Another important point in assessing the incidence of this form of ulceration is the time factor, for in a further group of cases the incidence rose from 16% after four years to 24% in a seven-year follow-up period. On the other hand, Lahey states that jejunal ulcer usually develops within two years of operation.

On the basis of the theory that a secretory substance is produced by the antrum of the stomach and passed to the glands of the fundus via the blood-stream, excision of the pylorus seems to be necessary, and several writers have pointed out that the incidence of anastomotic ulcer is much lower when the antrum and pylorus are removed.

One of the most dreaded complications of anastomotic ulcer is a gastro-colic fistula, really more correctly termed a gastro-jejuno-colic fistula. I might add that the operative treatment of this complication is very difficult.

The treatment of anastomotic ulcer itself is difficult, and in any case many patients refuse further interference. If the secondary ulceration has occurred after a gastro-enterostomy, it is probably advisable to undo the anastomosis and restore the organs to their normal state. If the acid curve is high, then ideally the anastomosis should be undone and a partial gastrectomy with a fresh anastomosis performed; but this is a formidable procedure. As was mentioned earlier in this paper, the new thoraco-abdominal incision renders such cases much easier to handle. In cases of marginal ulceration after a partial gastrectomy it is frequently stated that further surgical interference can rarely help. It is my own opinion that in these cases the attack should be made through a thoraco-abdominal incision when the anastomosis is undone and the rest of the stomach or almost all of it is removed and a fresh anastomosis is performed between the lower end of the œsophagus or the remnant of the stomach and the jejunum.

Incidence of Malignant Change in Presumed Gastric Ulcer as an Argument for Gastric Resection.

A review of the literature shows that various writers have found malignant changes in presumed gastric ulcer in from 3% to 15% of their cases. If the larger figures are accepted, then they are a strong argument for resection, for this means that a greater number of patients will be saved and the necessarily higher operative mortality rate will be justified.

In my own series of cases I have performed a gastro-enterostomy for frank pyloric stenosis in about 60 cases; in three cases the patients were examined two to four years later, each having a carcinoma which was inoperable and which soon led to death. If one presumes that pyloric stenosis is in some cases caused by a pyloric ulcer and in other cases by a duodenal ulcer, then the incidence of malignant change in pyloric ulcer with stenosis would be of the order of 8% to 15%. If these small numbers are significant, then they are a strong argument in favour of partial gastrectomy for pyloric stenosis; on the other hand, one could quote numerous series of patients with gastric ulcer who have been treated conservatively, and who have been followed up for periods of up to ten years, in which the incidence of secondary malignant disease has been extremely low.

Again, in the three cases of pyloric stenosis quoted in which malignant change later developed, the age factor was important, for the patients were over sixty years of age. I think all surgeons are agreed that a gastro-enterostomy gives excellent results in frank stenosis. Therefore perhaps a correct summing up of the position is that a posterior gastro-enterostomy should be performed on all patients up to about fifty years of age when there is no clinical or operative suggestion of malignant disease. I may add that it is often difficult to be sure at the time of operation whether an ulcer in the region of the pylorus has originated in the duodenum or in the stomach. I have now decided that for patients over fifty years of age with stenosis a partial gastrectomy is the treatment of choice, and as the resection need be only limited, I do not think that the mortality rate will be appreciably increased as compared with that for gastro-enterostomy.

Whatever may be the real incidence of malignant change, in old standing gastric ulcers, I think the procedure advocated earlier is the correct one—namely, a course of medical treatment for about six months, and if there is then no evidence of healing, the performance of an adequate gastrectomy. If there is any suggestion of malignant disease in any case at any time—that is, in less than six months—the patient should of course be subjected to operation.

Summary.

According to present ideas, I think that the main points in the treatment of peptic ulceration are briefly as follows:

1. Surgical treatment should be planned on a physiological basis.
2. Patients with uncomplicated duodenal and gastric ulcers should first receive an extended trial of proper medical treatment. If the response is not satisfactory, then a partial gastrectomy of adequate dimensions is desirable.
3. For pyloric stenosis a gastro-enterostomy is the treatment of choice if the patient is young. For the older subject a partial gastrectomy may obviate the risk of later malignant change.
4. In very severe hæmorrhage operation is advisable; if more than two less severe hæmorrhages occur, then again resort should be had to surgery.
5. For perforated ulcers the treatment should be early operation and suture, apart from an occasional bad risk subject, and perhaps conservative methods are justifiable if the patient is not examined for twelve to sixteen hours after the perforation has occurred.
6. Physiological gastrectomy is still a doubtful method of treatment.
7. Most patients with carcinoma of the stomach, if in operable condition, should have the most radical excision possible performed, the thoraco-abdominal approach being used.
8. Hourglass stomach can probably be treated by one of several operations; but resection above the constriction is generally the best.
9. Vagotomy is still under trial.

A BASIC ROUTINE FOR POST-OPERATIVE TREATMENT AFTER LAPAROTOMY.

By LEO DOYLE,
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THE post-operative treatment of patients who have undergone a laparotomy is a fruitful source of difference of opinion. I think that if consideration is given to a few facts some of the differences will quickly resolve themselves.

The facts are these.

1. The length of time for which the patient requires earnest consideration is in direct proportion to the seriousness of his operation and to the extent to which his small intestine has been handled or is involved in an inflammatory process.

2. The post-operative period is one in which the patient experiences pain, about which he complains to a greater or less degree. Absence of complaint does not mean absence of suffering any more than loudness of complaint is an indication of depth of suffering. This suffering can be mental as well as physical and can last for several days.

3. Thirst is the next important complaint made by most patients. This is quite understandable for several reasons. Firstly, pre-operative preparation may have included an aperient with its consequent fluid loss in stools. Post-operative vomiting not only causes fluid loss but it also prevents the patient from taking fluid. Should there be any fever, skin action may again involve some loss. Lastly, the operation itself may have involved a fluid loss of probably much greater import than those mentioned.

4. One of the most important observations made by Owen Wangensteen concerns the origin of gaseous distension. He has shown that the greater part of the gas comes from air swallowed by the patient, and consists mostly of nitrogen, as the accompanying oxygen is absorbed by the blood. A small proportion consists of gases arising from other sources. It also remains to be realized that the first thing done by anyone who has any abdominal distress is to swallow air, and that a patient will keep on swallowing it whilst any discomfort exists. If it is belched back no great harm is done and in some cases some satisfaction ensues; but if it is not rejected orally, it must pass through the entire length of the alimentary canal.

5. If one can now accept as a fact the statement that after handling or exposure the small intestine goes into a state of greater or lesser paralysis, which is not entirely proportional to the amount of trauma inflicted, many things are capable of explanation. Before we proceed with this line of thought a physiological fact must also be emphasized. It is that there is no absorption of water in the stomach and almost none in the small intestine. Most of the water must be transported through the twenty-odd feet of small intestine before it can help to relieve the thirst complained of by the patient. Therefore, if the bowel is suffering from any degree of paralysis, it is evident that water taken by mouth will not help to relieve the accompanying thirst, but will only increase the general discomfort. If this has not been adequately treated by morphine, the patient will seek relief as indicated above by swallowing air, thus succeeding only in making matters worse. This state of affairs has only to progress a little further and be helped by further injudicious draughts of so-called non-nauseating drinks and brought to a head by aperients or pituitrin, and we have a full-fledged paralytic ileus on our hands.

The Patient's Complaints in the Post-Operative Period.

Thus we see that the post-operative condition is one in which the patient may complain of pain which can be relieved by morphine, of thirst which cannot be relieved by drinks, of vomiting which will increase his thirst, and of distension brought about by air swallowing. If these facts are appreciated and appropriate measures taken, the post-operative period can be very easy.

Pain must not be left untreated, and morphine should be given in adequate doses and at frequent intervals. I think that anything less than one-quarter of a grain of morphine is insufficient as a first dose for an average adult. Morphine (for example, one-sixth or one-eighth of a grain) should then be given at intervals of four hours or more often if this is thought necessary. The patient should not be allowed to suffer pain when it can be relieved by morphine. No pain, no air swallowing; no air swallowing, no distension.

Thirst must be relieved, and this cannot be achieved by drinks. The easiest way is to give tap-water by means of a rectal drip apparatus. It must be tap-water—neither saline nor glucose solution. These two are not absorbed, whilst tap-water is. Tap-water will be absorbed in quantities sufficient to relieve the body needs, and is given by a very simple piece of mechanism. If for any reason it is impracticable or impossible to give the water by rectum, fluid must be given parenterally. It must not be given by mouth, as the paralysed small intestine is incapable of transporting it to the large bowel, where it is absorbed.

So it seems that the two worst features of the immediate post-operative period are pain and thirst. If these are treated by very simple methods, most of the usual post-operative troubles will be avoided and paralytic ileus only a bad memory. If the lesion has involved a serious operation or is associated with any degree of peritonitis, this *régime* can be persisted with for three to four or more days; but for an appendicectomy or any minor procedure twenty-four to thirty-six hours will be sufficient.

This subject cannot be left without reference to the treatment of the bowels. Whilst this *régime* is being pursued there should be no bowel treatment of any kind. It should be realized that the period of illness will be associated with a small food intake which also will probably have been of a low-residue type, so that there cannot be a large amount of fæcal content to be passed. Leave the bowels alone; give no treatment and all will be well. The bowel will act in due course without assistance. Infrequently, after a major intervention, when there has been some dehydration, a patient will have a fæcal impaction. This should not occur if the fluid level has been kept up; but if it should occur, it can be relieved in the usual fashion.

I am quite sure that if these simple rules are followed they will give patients a painless post-operative period and relieve the surgeon of much anxiety.

Quantity of Fluid to be Administered.

The daily amount of fluid usually required is from 2500 to 3000 millilitres. This latter amount, at least, should be given in the first twenty-four hours of the post-operative period. It should also be remembered that the day of operation will also have been one of not only diminished intake, but also probably of fluid loss during operation. An endeavour should be made to assess this loss, and the amount of the loss should be made up during the first twenty-four to forty-eight post-operative hours. When this fluid is given by a rectal drip administration of tap-water, the method is nearly fool-proof; but if it has to be given parenterally some considerations are necessary. Firstly, it is important not to give too much sodium chloride. This may be avoided quite simply either by giving intravenously not more than one litre of saline solution per day and giving the rest of the fluid requirement as 5% dextrose solution, or by using only 4% dextrose solution and one-fifth normal saline solution. (This mixture is made and on the market as a "Soluvac" preparation.) Another consideration is that in the case of elderly people it is probably better to have their circulatory system a little deficient rather than overloaded. Should it be necessary to keep giving fluids intravenously for some days, watch should be kept to see that the plasma protein does not drop below the œdema level. Should œdema be noticed, it should be treated by the intravenous administration of plasma if the better way of giving protein by mouth is not available. If a patient can take it, the giving of red meat by mouth is the best and quickest way of raising the plasma protein level.

I also feel that an adequate fluid intake in the post-operative period is an important method of preventing hæmoconcentration, with its danger of intravascular clotting.

Other Measures.

The post-operative use of a 5% mixture of carbon dioxide in oxygen has the important effect of making the patient ventilate his lungs freely in the immediate post-operative period should he be unconscious; but after this period has passed, oxygen inhalations are useful for some patients. Whatever their effect may be on the blood-stream oxygen level, they make it likely that any gas swallowed will be capable of absorption and will not cause an addition to any existing distension. In the early post-operative period it is also a good practice to have the nurse insist that the patient coughs frequently and to see that he does breathing exercises consisting of taking twelve deep breaths every hour.

Recently I have been using non-absorbable sutures and encouraging patients to move freely in the bed. They are invited to sleep on their side and to move their legs freely. They are encouraged to sit up in bed, and as soon as they achieve this without trouble they are invited to sit out of bed and then walk a little. By encouragement many patients can be out of bed for their first bowel action, thus avoiding the bed-pan gymnastics which are such a terror to so many.

Morphine apparently produces nausea in some patients, and when this occurs, or is suspected, "Omnopon" or heroin may be substituted.

Summary.

Treat post-operative pain and thirst by morphine and the rectal or intravenous administration of fluids. Do not interfere with the bowels. Keep the patient moving freely in bed. Let him out of bed as soon as is reasonable, comfortable and acceptable. Do not force him out of bed.

These short rules constitute a foundation on which the post-operative treatment of all patients subjected to laparotomy can be founded. They are varied and supplemented as indicated to suit the individual patient. They are founded on established facts and conflict with very few of the accepted rules. In my cases they have given a great deal of satisfaction and saved me much anxiety.

Reviews.

A BOOK ON PROBLEMS IN OBSTETRICS.

"PRACTICAL OBSTETRICS", by Professor Bruce T. Mayes, is not an easy book to review as it does not conform to the usual formulæ of textbook writing.[1] But then it is not a textbook, a fact made clear and emphasized by the author in both the preface and the epilogue. It therefore should be approached in the spirit of the words of Pope:

> In every work regard the author's end,
> Since none can compass more than they intend.

The author's end is to provide accessible, sound advice to graduates who presumably are familiar with the basic principles of obstetrics and who would find tedious and obscuring any repetition of the minutiæ which harassed their student days. This end he has undoubtedly reached.

The genesis of the book is of interest since it stems from a series of "Bulletins on Obstetrics" for wartime graduates in the services which the author prepared in a praiseworthy effort to keep absent graduates in touch with clinical work in civilian practice.

In twenty-two chapters Professor Mayes discusses in a friendly, colloquial and aphoristic manner, not devoid of jargon, most of the difficulties, with a few notable exceptions, likely to confront the practitioner in the early years of private practice, using as his texts actual cases with both happy and unhappy endings. It would be easy to criticize this diversion from the orthodox were it just to do so, but, as already stated, the intention is clear.

[1] "Practical Obstetrics", by Bruce T. Mayes, M.V.O., M.B., B.S. (Sydney), F.R.C.S. (Edinburgh), F.R.A.C.S., F.R.C.O.G.; 1947. Sydney: The Australasian Publishing Company Proprietary, Limited. 9" x 6", pp. 322, with many illustrations, some of them coloured. Price: 57s. 6d.

Oddly enough the book opens with a chapter on one of the terminal dangers of parturition, post-partum hæmorrhage. The clarity of the advice for control of bleeding is salutary, for the prompt and vigorous application of simple procedures is the keynote of success. Few today will dispute the advocacy of relatively early manual removal of the placenta when bleeding continues: many lives have been lost from postponing this manœuvre until the patient is approaching a state of irreversible shock. As the author states we should regard manual removal of the placenta with respect, but not with awe. The advice to follow the removal with a hot intrauterine douche is more vulnerable, and this unnecessary and probably useless addition has long been given up in many clinics. In these days when students are well accustomed to intravenous medication by a slow drip method it is wise advice to insist on rapid transfusion for post-partum hæmorrhage and shock. The condition is one of acute cardio-vascular collapse which can be adequately combated only by immediate and rapid replacement.

Breech delivery is dealt with along orthodox lines with an added description of the Lovsett method of delivering extended arms. It is, however, doubtful if the unfortunate fatal case of breech delivery of a hydrocephalic infant, a rare occurrence, is well chosen as the text; more suitable would have been a difficult but successful primiparous breech delivery. Most obstetricians would be unhappy following the author's dictum that a delay of twenty minutes after birth to the level of the umbilicus is a safe margin for survival of the infant. Assuming that the funic vessels are compressed, the fœtus is in a state identical with that of a person immersed in water; resuscitation after twenty minutes would be doubtful. In Australasia, where serious pelvic contraction is occasional, the occipito-posterior position of the head is probably the commonest single cause of difficult labour. This complication is well dealt with in Chapter V with proper emphasis on manual in preference to instrumental rotation, the value of the fœtal ear for diagnosis and the fact that in most cases the fœtus rotates spontaneously if only allowed to do so. The forceps operation is described with admirable clarity and illustrations. Again the illustrative case might be better chosen since the unfortunate one used confuses the issue between the danger of extravagant use of calcium during pregnancy and the problems of forceps delivery. Although Professor Mayes's book is not a textbook, space could with advantage have been found for the indications for the use of forceps.

Chapter VII, with a precision seldom found in textbooks, gives adequate advice for the conduct of primary inertia and long labour, a condition which harasses equally the patient, doctors and the relatives. It is not easy to understand the dogmatism of "Never put them [Willett's forceps] on a dead baby", certainly not on a macerated fœtus, but there is no objection to their use on one recently dead. Mention could well be made of gas and air anæsthesia which many clinics find of value. In a book of this kind the problematical and experimental use of diathermy for the slowly dilating cervix could well be omitted. Very proper caution of the dangers of cervical incisions is expressed.

The excellent description of the lower segment Cæsarean section leaves nothing to be desired, although there is a minor error in the reference on page 121 to Lois Benson's article in THE MEDICAL JOURNAL OF AUSTRALIA; it appeared in the issue of June 15, 1946, and not as stated in that of June 5.

One of the most valuable sections of the book are the three chapters on toxæmia of pregnancy and its terminal catastrophe of eclampsia. Professor Mayes insists, in a way that might well be copied by other teachers and textbooks, on the imperative need for early treatment as well as early diagnosis. He points out that recognition of the significance of weight increase and hypertension gives warning well ahead of critical levels. He implies, a doctrine which cannot be too strongly advocated, that, with negligible exceptions, eclampsia is a preventible disaster and that its occurrence in a patient whose pregnancy the doctor has had the opportunity to supervise must be written down as a failure. The importance is stressed not only of early diagnosis but of prompt treatment and of the impossibility, under modern social conditions, of adequate treatment in the home. There is a refreshing simplicity about diet compared with the highly scientific but often impractical formulæ of dietitians. The well-recognized conservative treatment of eclampsia is set out so that the practitioner confronted with this alarming and panic-creating event can grasp quickly the fundamental principles. If the advice in these chapters were taken there would be few cases of eclampsia and few occasions when the term "fulminating toxæmia" could be justly used. It is true that the final catastrophe can come with dramatic suddenness, but warning of weeks or more is given to those who seek. Although it may be difficult to obtain hospital

accommodation for toxæmic women who do not appear subjectively ill, this embarrassment does not affect the wisdom and truth of the advice. These chapters should help to give the *coup de grâce* to the "testtube minded" doctor and nurse who still regard albuminuria as the only sign of toxæmia worthy of notice. On page 163 the discussion on protein intake in toxæmia includes the following: "The loss of protein (albumin) in the urine is a thoroughly logical reason for eliminating its intake." Surely there should be a negative in this sentence. A number of minor problems are discussed with the directness which characterizes the whole book, local anæsthesia to the perineum, the episiotomy incision, induction of labour and so forth. There are two really good chapters on sterility and an excellent exposition of the Rh factor in pregnancy, ample for the needs of the obstetrician.

On page 68 Professor Mayes remarks: "It is not my policy to engage you in a scientific study of theory. In telling you these stories I have constantly before my mind the young doctor going into practice and wanting to know how to deal practically with the problems of obstetrics. When it comes to dealing with the individual patient and her baby as your responsibility there is no room for theorizing." This intention is fully accomplished.

This is a book to be welcomed, read and re-read by all young practitioners entering practice and especially by those whom war service removed from practical clinical obstetrics. It contains both the finger-post to success and the red lamp of danger. Although we are promised more in the epilogue, this book would have been a rounder volume if abortion, ante-partum hæmorrhage and puerperal sepsis had been included.

Great credit is due to the artist for many admirable illustrations and to the publishers for the quality of their work.

INTER-ALLIED CONFERENCES ON WAR MEDICINE.

IN December, 1942, the Royal Society of Medicine of London initiated a series of inter-Allied conferences on war medicine, with the approval of the heads of the medical services. For security reasons these were restricted to officers of the services, and no reports were published till the present time.[1] Although the collected reports are not complete, they are of great interest, since they summarize the personal views and experiences of responsible Allied officers, given when the details were fresh in their minds. The subjects include practically all aspects of war medicine from organization to recent technical advances, and in their compressed yet personal style emphasize the great range of work done. The book deserves a wide circulation.

BIOCHEMISTRY FOR MEDICAL STUDENTS.

IN its fourth edition Thorpe's well-known "Biochemistry for Medical Students" has been increased by some twenty pages.[2] Most of the chapters have been revised. A short chapter on the use of isotopes has been added, and there are many references in the text to their application in following the course of metabolic changes in the body.

The chapter on protein structure now includes only the briefest reference to Wrinch's cyclol hypothesis. This treatment is no doubt justified on the basis of certain objections which have been raised against the hypothesis. No alternative has been put in its place (for example, the stacked hexagons hypothesis of Huggins) and the student is left in the air with regard to the possible structure of the corpuscular protein molecules. The author still uses the term globular for these proteins with its unjustified implication of spherical or spheroidal form. In the section on nucleoproteins no reference is made to the extranuclear occurrence of these proteins.

The discussion of digestion of protein is disappointing. Bergmann's classification of the peptidases is not considered. There is no discussion of the points of attack on the protein molecule of the various peptidases which are now recognized.

The section on muscle metabolism is also disappointing. The Krebs or tricarboxylic acid cycle of the aerobic phase of carbohydrate metabolism in the muscle is not mentioned.

The chapter on vitamins would have benefited by further revision. For example, something more than mere mention of biotin and folic acid might have been expected.

A considerable portion of the book is devoted to various aspects of nutrition. Numerous tables of food values are included. The value of statements of food costs in a textbook of this kind is doubtful. Parts of this section have obviously been influenced by the pressing, but it is to be hoped transient, problems of food supply with which Great Britain is at present faced. In the section on energy requirements the very incomplete and misleading graph of basal energy requirements at different ages has escaped revision.

The book contains a short but useful list of selected references to recent literature on many of the topics discussed.

AN ATLAS OF CARDIO-VASCULAR DISEASES.

TREIGER, in the preface to his "Atlas of Cardiovascular Diseases",[1] stresses that "correlation of history, physical findings, roentgenogram and electrocardiogram leads to a correct understanding of the degree of anatomical and physiological changes in the heart and the proper dose of various medication for their correction. Regardless of how convincing one method of examination may be, the interpretation of it without correlation with other findings may lead to mistakes and failure of therapy". A study of his book shows that the author has attained his object. The atlas is divided into six parts: "Normal Heart", "Rheumatic Disease", "Arteriosclerotic Heart Disease", "Hypertension", "Syphilitic Heart Disease", and "Congenital Anomalies". In each chapter the relevant points of the history and the clinical findings are discussed, skiagrams taken with the patient in one or several positions are presented and pathological specimens are demonstrated. An electrocardiogram usually completes the demonstration of each case. A summary correlates the various findings. The radiographs are of an excellent quality, and the photographs of pathological specimens—some of them in colour—have that technical perfection which enables the reader immediately to recognize the lesion presented. Particularly impressive are the photographs of valvular lesions, of infarction of the myocardium and of congenital abnormalities.

The majority of the electrocardiograms are also perfect in quality; a reader not familiar with electrocardiographic tracings might find it difficult to read some of them. The expert, however, will appreciate the difficulties in obtaining perfect curves in the case of very ill patients. The instability of the baseline and the superimposing of alternating current waves will not be disturbing for the expert.

Each chapter is opened with a detailed discussion of the general problem, for instance, in the chapter on rheumatic heart disease pathological changes in typical cases are fully explained. The active stage with its clinical, radiological and electrocardiographic findings is contrasted with the inactive stage. Typical lesions of the mitral valve and of the aortic valve and cases of mixed valvulitis are demonstrated.

Particularly valuable is the chapter on congenital anomalies of the heart. The photographs of pathological specimens are as clear and impressive from a didactic point of view as any ever seen in an atlas of heart diseases.

The author does not aim at presenting complicated cases. His presentation of simple, uncomplicated cases of cardiac diseases makes it much easier to analyse the complicated anatomical and physiological changes of some cardiac diseases with more than one ætiological factor.

It is fortunate that there is an increasing number of publications presenting the correlation of history with physical, radiological and electrocardiographic findings. The author with his "Atlas" has added a valuable and instructive book to these publications which restore the necessary balance of modern methods. Therefore this book will be welcomed by the clinician who tries to master these modern methods.

[1] "Inter-Allied Conferences on War Medicine, 1942-1945; Convened by the Royal Society of Medicine"; honorary editor, Major-General Sir Henry Letheby Tidy, K.B.E., M.D., assistant editor, J. M. Browne Kutschbach, M.B., B.Ch., D.P.H.; 1947. New York and Toronto: Staples Press, Limited. 8½" × 5½", pp. 532, with illustrations. Price: 50s.

[2] "Biochemistry for Medical Students", by William Veale Thorpe, M.A. (Cantab.), Ph.D. (London); Fourth Edition; 1947. London: J. and A. Churchill, Limited. 8" × 5½", pp. 504. Price: 18s.

[1] "Atlas of Cardiovascular Diseases: Correlation of Clinical Electrocardiography and Cardiac Roentgenology with Clinical History and Autopsy Findings", by Irving J. Treiger, M.D.; 1947. St. Louis: The C. V. Mosby Company. Melbourne: W. Ramsay (Surgical) Proprietary, Limited. 11½" × 8½", pp. 186, with many illustrations, some of them coloured. Price 75s.

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HOMOSEXUALITY.

HOMOSEXUALITY has for long presented a problem to medical practitioners, psychologists and lawyers. During recent years it has been mentioned more freely in general literature than it was a few decades ago. This change, which is a manifestation of the increased freedom of expression in the world of today, is all to the good if it leads to an understanding of difficult problems. It is not so long ago since the first works of Havelock Ellis were regarded by officialdom with anything but favour, his book, "Studies in the Psychology of Sex: Sexual Inversion", being described as a "wicked bawdy scandalous and obscene libel". The study of homosexuality is important from two points of view—first of all, the effect which homosexuals and homosexuality may have on society, and secondly, the attitude which society should adopt towards its homosexual members. If the subject is dealt with at all by medical practitioners both these aspects should be given full consideration. This is not always done. In April, 1947, a discussion on the social aspects of homosexuality took place at a meeting of the Section of Psychiatry of the Royal Society of Medicine, and a summary was published in the *Proceedings* of that body for August of the same year. Discussion had to do chiefly with society in relation to homosexual persons and the criminological aspect of the question.

The first fact to be borne in mind when this subject is considered is that every degree of homosexuality is found among homosexuals, just in the same way as heterosexual characteristics present enormous variations among the heterosexual persons in the community. Medical practitioners do not need to be reminded that homosexual behaviour of a gross kind which brings an individual into the hands of the police does not characterize the majority of homosexual persons; the laity should be made to realize this fact. Non-medical persons know quite well that heterosexual persons may allow their sexual inclinations to become manifest in reprehensible and revolting actions. It is the acts of extreme homosexual persons which are largely responsible for the antipathy of normally sexed people to homosexuals. There are other reasons, and prominent among them is an inherent dislike of the abnormal in personal make-up and behaviour. Closely linked with this is the reproductive instinct, the life of the family and all that goes with it. E. A. Bennet in the London discussion suggested that the average man thought that if he did not condemn homosexuality, or scoff at it, it might be thought that he approved of it. Bennet holds that the opinion of the man-in-the-street in Great Britain on homosexuality is emotionally tinged, subjective and largely determined by hostile prejudice. Unfortunately the extent of homosexuality is not known. Bennet quotes Havelock Ellis that the inverts in Britain number slightly over 2% of the population, or over a million. Ellis in his "Psychology of Sex" states that Hirschfeld in Germany showed that a number of separate estimates among different classes of the population revealed a proportion of inverted and bisexual persons varying between 1% and 5%. Ellis found that homosexuality was scarcely less common among women than among men. Bennet quotes Myerson and Neustadt who wrote in America in 1942 that statistics revealed a variation from "as high as more than 50% of an unselected population to as low as 2%". Katherine B. Davis reported from New York in 1929 that 26% of 1200 unmarried college women graduates had had intense emotional relationships accompanied by overt physical practices with other women. With such divergent estimates and with different conditions found in different countries we may agree with Bennet that it would be rash to draw conclusions about the incidence of homosexuality in Britain and we may extend the statement to Australia as well.

Homosexuality, or to give it its other name, sexual inversion, has been divided or classified into groups. Dr. Stanley-Jones at the London discussion said that the groups were referred to as congenital and acquired, or organic and functional, or physiological and psychological. Unless some grouping of this kind is recognized, the problem of homosexuality cannot be understood. The homosexual manifestations of adolescence are often temporary and have no permanent effect on the individual; they must be regarded differently from the congenital type which probably has an endocrine basis. The male congenital invert has been said to have "a female soul in a male body". There are several theories of the origin of homosexuality; we need not concern ourselves with these. Suffice it to remark that the condition is to be regarded as an anomaly, a "variation of the normal sexual pattern". Havelock Ellis wrote in his "Psychology of Sex" (1933, page 208): "Congenital sexual inversion is an anomaly, an inborn variation of which we are beginning to understand the causes; it is, even when extreme, only pathological in the same sense as colour-blindness or albinism or transposition of the viscera is pathological." In regard to pseudo-homosexual persons Stanley-Jones believes that "the persistence into adult life of the homosexual tendencies that are natural and indeed universal during adolescence is symptomatic of a failure of development, an arrest of normal psychological growth at the pre-adolescent stage of homosexuality, rather than of the divergence of inborn and unalterable traits that inform the basic pattern of an abnormal personality". The recognition of the different types of homosexuality is important when we discuss treatment. With treatment punishment must unfortunately be mentioned. If we take the latter first, we must note Stanley-Jones's contention

that by far the greater number of wrongdoers who are actually charged with homosexual offences are not natural inverts in the medical sense of the term, but merely unnatural heterosexuals or bisexuals. Sir Norwood East expressed the opinion at the discussion that in certain of these cases imprisonment, even without psychotherapy, might have a curative effect, and that in specially selected cases psychotherapy had a place in treatment. The experience of others in this regard might be quoted if space was available. East said that homosexuality in men should be penalized if it led to the debauching of young people, if it offended against public decency, if it was carried out for monetary gain, or if the homosexual was in any public place for the purpose of prostitution, or if he solicited to the annoyance of passengers. In regard to adult homosexuals of the "congenital" group, treatment is better not attempted. As long as such persons keep to themselves and do not seek to influence young persons and to lead them along homosexual paths, they should not be interfered with. There is something to be said for the idea, stated by Stanley-Jones as characteristic of the trend of advanced medical thought, that legal and administrative opinion should be educated towards a complete separation of the two categories, and that the true homosexual should be "rescued from the clutches of the law which sometimes operates with mediæval barbarity". Whether society will ever be sufficiently enlightened to do this is doubtful; but in the meantime some reliance can, we hope, be placed on the common sense of the police and others who administer the law.

The last aspect of this subject and in some respects the most important has to do with the attitude of society to homosexual persons and with what they can do to help themselves. It should be clear from the present discussion that what is needed from the point of view of the unfortunate and lonely subjects of inversion is understanding. Their plight is real and their course difficult. An insight into the sufferings of these unfortunates may be gained from perusal of "The Invert", by "Anomaly", a book reviewed in this journal in December, 1927. In many ways this book was remarkable. The author was not medically trained, but he wrote with insight and from personal knowledge. "Anomaly" made a plea for understanding; he wrote of the unhappiness of the invert and of his need for life-long self-control. Incidentally he made an appeal to other inverts for self-reliance and continence. "Anomaly" held that the problem of invert morality was part of the general question of sexual morality, as indeed it is. Another book bearing on this subject is "The Single Woman and Her Emotional Problems", by Laura Hutton, which was reviewed in this journal in July, 1938. *The Bulletin* in 1927 in a "Red Page" article on "The Invert" quoted what has been written of "self-reverence, self-knowledge, self-control", and remarked that it applied to homosexuals and heterosexuals alike. "The building of character depends chiefly on self-control, and it follows that the homosexual, from the very fact that he has to fight for self-control more than any other person, may possibly achieve strength of character surpassing the average." When this happens we may say that a burden has (though at enormous and never-ending cost) been turned into an asset. The psychologists talk of sublimation, and we know from our days in the chemical laboratory what a disturbing process it may be in a test tube. It is more than this in a man. While this difficult process is being attempted by any person a tolerant and understanding attitude by sympathetic neighbours cannot be other than helpful. Most intelligent people practise sublimation, though they may not know it by that name.

Current Comment.

MONTEGGIA FRACTURES.

GIOVANNI BATTISTA MONTEGGIA, of Milan, described in 1814 what has come to be known as the Monteggia fracture. Although over a century has elapsed, and the X-ray machine has done so much to facilitate the precise investigation of injuries to bones and joints, there is evidence to suggest that the full implications of the injury described by Monteggia are not universally appreciated. Because of this lack of understanding errors are likely to be made even after examination by means of X rays. The Monteggia fracture (or, more exactly fracture and dislocation) follows direct violence to the forearm of such a nature and degree as to fracture the shaft of the ulna, usually in its upper third. The violence is also transmitted through the interosseous membrane to the radius as a traction force and either drags the head of that bone out of the orbicular ligament or tears the latter. The fracture of the ulna is not likely to be overlooked, but the radial displacement may pass unnoticed, and therefore untreated. If this occurs the prospects of good functional recovery may be gravely prejudiced.

Attention has been drawn to this problem by Frederick Smith,[1] who has prepared an analysis of twenty-five consecutive fresh Monteggia fractures treated between 1929 and 1944. This author stresses the importance of recognizing the full significance and implications of this injury, and avers that in forearm injuries when the ulnar shaft alone is fractured, it should be axiomatic to assume a dislocation of the radial head until confirmed or disproved. In the lateral X-ray picture of the elbow a line prolonging the axis of the radial shaft should pass through the centre of the capitellum of the humerus, or through its epiphyseal centre, no matter what the position of the elbow as regards flexion or extension. Most frequently the ulnar fragments are angulated anteriorly and the radial head dislocated anteriorly also. Less often the angulation and dislocation are both lateral. Watson-Jones in his book on fractures and other bone and joint injuries has described posterior angulation and dislocation, but Smith has not encountered this form of displacement.

Monteggia fractures are far more common in children than in adults, the series of 25 cases under analysis comprising three adults, one fifteen-year-old child and one twelve-year-old child, and twenty children between the ages of nine years and three years. The injury may be complicated by paralysis of the posterior interosseus nerve, a condition which, in Smith's experience, usually improves spontaneously. Successful treatment depends firstly on the recognition of the true nature of the injury, and secondly on the correction of both the dislocation and the fracture. Watson-Jones states that recent Monteggia fractures are easily reduced by manipulation, provided this is carried out within a few days of the injury. Smith's view differs from this, and he finds that no single method is applicable to all cases. In some instances manipulation by a closed method will succeed. With the elbow extended and under strong traction, backward pressure is exerted on the fragments and on the radial head. If displacement is successfully overcome, and the corrected position can be maintained (which will usually depend on whether the ulnar fragments "lock"), the elbow is immobilized at a right angle. Check X-ray examinations of the fracture and of the radial head should be made once or twice each

[1] *Surgery, Gynecology and Obstetrics*, November, 1947.

week. If either becomes redisplaced the author considers that open operation should be undertaken. In some instances, in Smith's experience, the orbicular ligament, or part of it, becomes interposed between the radial head and the radial notch of the ulna. When this occurs open operation must usually be undertaken before the dislocation can be corrected. The cases which lend themselves to closed reduction are those in which there is no ligamentous barrier to replacement of the radial head, and in which the ulnar fragments will lock. Smith has observed a tendency for these fragments to angulate after reduction, and in adults to develop non-union or malunion. He therefore advises open operation in all adults, with plating of the shaft of the ulna, and repair of the orbicular ligament. Smith considers that it is justifiable to attempt closed reduction in children, but if this is not perfect, or is not maintained, then within ten to fourteen days of injury, operative correction and fixation should be carried out.

The Monteggia fracture is not a common injury, but neither is it a rare one. Awareness of its existence will constitute the greatest aid in recognizing this condition.

BACTERIAL RESISTANCE TO ANTIBIOTICS.

THE possibility that organisms will develop resistance to a particular therapeutic agent has always to be borne in mind, though nowadays it is more likely to be appreciated by the bacteriologist before it becomes important to the clinician. Some interesting facts regarding the development by organisms of resistance to penicillin and streptomycin have been recently presented by C. Phillip Miller of the University of Chicago.[1] Among penicillin-susceptible bacteria strains of naturally resistant organisms are uncommon, though some strains of staphylococci are comparatively resistant and a few (which produce the penicillin-inhibiting enzyme penicillinase) are highly resistant. Acquired resistance to penicillin is seldom seen clinically in man, firstly, because it develops so slowly that there is not sufficient time before the infection is brought under control and, secondly, because such large doses of penicillin are usually given nowadays that a moderate degree of increased resistance would be overcome. Miller points out that the mistake must be avoided of diagnosing acquired penicillin resistance when failure of an infection to respond to penicillin therapy is actually due to secondary invasion by a penicillin-resistant organism. Although reports of acquired resistance to penicillin in man are few, the phenomenon has been clearly demonstrated experimentally: *in vitro* by multiple transfers of the organisms on to media containing increasing concentrations of the drug; *in vivo* by repeated passage of the organisms through animals treated with subcurative doses of the drug. The latter suggests a possible danger in the clinical field, but the low toxicity and the ready availability of penicillin provide no excuse for inadequate dosage.

By contrast, Miller points out that resistance to streptomycin may be acquired rapidly. This has been demonstrated *in vitro* and also clinically; infections of the urinary tract, for example, must be controlled within a few days or they will become completely resistant to the maximum tolerated dose of streptomycin. In the investigation of this phenomenon it has been found that two resistant variants of the meningococcus develop, which appear during the initial cultivation of a susceptible strain on media containing streptomycin in excess of ordinary bactericidal concentrations. Both variants are completely resistant to streptomycin and one of them actually requires streptomycin for its growth *in vitro* and *in vivo*. This second variant is developed constantly when the right concentration of drug is used in the medium; it dies if transferred to a medium which does not contain streptomycin, and it produces infection only in an animal which is being treated with streptomycin; otherwise, however, it retains the properties of normal meningococci including sensitivity to penicillin. A strain of brucella has also been found which grows better in the presence of streptomycin, but can be grown in its absence. Most of these facts are of significance only to the bacteriologist, and it is to be hoped that the clinician's interest in them may remain mainly academic.

[1] *The Journal of the American Medical Association*, November 22, 1947.

TOTAL GASTRECTOMY.

CANCER of the stomach is responsible for more deaths each year than cancer of any other organ. This statement epitomizes the unsatisfactory state of affairs in regard to the management of the condition. The grim prospect facing a patient admitted to hospital with a diagnosis of gastric carcinoma was emphasized by Ogilvie[1] in an analysis of 200 cases. In these cases 50% of the lesions were obviously inoperable on clinical examination. Exploration of the peritoneal cavity in the remainder showed that in only 20% could radical removal be performed. Finally, of the 200 patients who comprised the series, only ten, or 5%, were alive at the end of five years.

What are the prospects of improving this position? While we are limited to the methods of treatment in use at present, it would seem that the attack must be first in the directions of earlier diagnosis, and second in that of more radical and yet safer surgery. Though we look forward hopefully to the time when newer and better weapons will be forged, possibly by the nuclear physicist or the biochemist, we must take stock of such opportunities as seem to offer a means of rescuing an increasing number of sufferers. One avenue which appears to deserve detailed consideration and perhaps further exploration, is the place of total gastrectomy in the treatment of gastric carcinoma. Introduced many years ago, this operation has been practised infrequently because of a forbidding mortality, but recent advances in technique, anæsthesia, and the control of infection, together with a better understanding of the underlying biochemical principles, seem to warrant a more optimistic attitude, and to indicate the need for a review of the circumstances which may be regarded as justifying so formidable a surgical enterprise. At the Lahey Clinic the mortality for total gastrectomy in the complete series of 92 cases over a period of years has been 29%, but in the last 43 cases the rate has fallen to 16%. It is claimed that for the last three years (number of cases unstated) no operative deaths have occurred. Figures showing a similar improvement have been published from other clinics.

Reginald H. Smithwick[2] has recently contributed a survey of total gastrectomy, and concludes that it seems proper, in view of the diminished risk, to use this operation more frequently, particularly in cases in which a lesser procedure may jeopardize the chance for cure because of an inadequate margin of safe resection beyond the gross limits of the disease. Smithwick considers that the noteworthy reduction in operative mortality is due principally to improvements in surgical technique, and quotes figures showing that in earlier series over half of the recorded deaths were due to peritonitis. Other technical details of importance include the removal of the spleen, the mobilization of the left lobe of the liver, the fixation of the jejunal loop to the diaphragm by an outer row of sutures, and the performance of an entero-anastomosis. Smithwick has been impressed by the value of performing œsophago-jejunostomy by a closed, aseptic method, and describes a clamp which he has devised for the purpose. This instrument has thin jaws which approximate in parallel fashion and ensure equally distributed pressure.

Linitis plastica affords an example of a malignant process in which, since the activity of the growth seems directed towards diffuse mural spread rather than distant metastasis, total gastrectomy may properly be advised and carried out.

[1] *The Lancet*, Volume II, 1938, page 235.
[2] *The New England Journal of Medicine*, July 10, 1947.

Abstracts from Medical Literature.

BACTERIOLOGY AND IMMUNOLOGY.

Penicillin Esters.

J. UNGAR (*The British Journal of Experimental Pathology*, April, 1947) has studied the activity *in vivo* of penicillin esters. These compounds themselves are biologically inactive, but can be quantitatively hydrolysed into free penicillin. The relative protective effect compared with sodium penicillin was studied in mice infected intraperitoneally with fifty lethal doses of hæmolytic streptococci, and in these animals both substances were completely protective. Similar results were obtained in rats and guinea-pigs, but rabbits were not protected, and penicillin could not be detected in rabbit serum or urine. It was therefore inferred that mice, rats and guinea-pigs possessed an esterase which was capable of hydrolysing the penicillin ester and liberating the active principle, but that rabbits did not. Four human volunteers were then tested, and results were obtained similar to those in rabbits, no free penicillin circulating in the blood, and very small traces being present in the urine. It was concluded that penicillin esters were unlikely to have any therapeutic value in man when given by the systemic or oral routes.

Infection of Mice with Tubercle Bacilli.

CYNTHIA PIERCE, R. J. DUBOS AND GARDNER MIDDLEBROOK (*The Journal of Experimental Medicine*, August, 1947) have studied the infection of mice with mammalian tubercle bacilli grown in Tween-albumin liquid medium. Two strains of mice were inoculated intravenously with graduated doses of the cultures, the inoculum being suspended in sterile culture medium. One strain succumbed to 0·02 millilitre, the other to 0·003 millilitre of seven-day culture, the inoculum always being a volume of 0·1 millilitre. Pulmonary lesions were present in all the surviving animals given smaller doses, while larger doses caused death in too short a time to establish visible lesions in the lungs. Intracerebral inoculation was also carried out with small graded doses, and early evidence of proliferation obtained with extremely small doses of culture. Infection by this route was also achieved with so small an amount as 0·01 millilitre of sputum from a tuberculous patient. Intraperitoneal injection of culture required a considerably larger dose to establish infection, but if the inoculum consisted of material from tuberculous embryonated hen's eggs, the infectivity of much higher dilutions of this material could be demonstrated. Normal egg yolk added to cultures in Tween-albumin also increased the infectivity, but if the egg yolk was given to the mouse separately from the culture, either before or after the bacilli, no modifying effect on the course of the disease could be demonstrated. Different strains of mice differed greatly in their susceptibility to infection, pigmented mice having less resistance than albinos. The authors discuss the importance of the factors influencing the course of the experimental disease in mice and the usefulness of young physiologically active cultures which can be obtained by the use of the liquid medium.

Nisin: A New Antibiotic.

A. T. R. MATTOCK AND A. HIRSCH (*The Lancet*, July 5, 1947) give an account of an antibiotic substance, nisin, isolated from liquid cultures of *Streptococcus lactis*, which is active *in vitro* against many types of pathogenic and other organisms, including the streptococci of groups A, B, E, F, G, H, K, M and N, staphylococcus, pneumococcus, neisseria, bacillus, clostridium, corynebacterium, actinomyces, mycobacterium and erysipelothrix.

Pneumotropic Virus and Pulmonary Tuberculosis in Mice.

MOGENS VOLKERT, CYNTHIA PIERCE, FRANK HORSFALL, JUNIOR, AND R. J. DUBOS (*The Journal of Experimental Medicine*, September, 1947) have studied the enhancing effect of concurrent infection with pneumotropic viruses on pulmonary tuberculosis in mice. The tubercle bacillus strain known as H37 RV, two different strains of virus, one of human, one of mouse origin, and two different strains of mice were used. Various experiments were set up in groups of mice, the infections being introduced in some cases simultaneously, in others separated by varying intervals. The virus was always introduced intranasally and the tubercle bacilli were always introduced intraperitoneally. The mouse lungs were all examined histologically; the number and extent of the lung lesions found were expressed in a numerical scoring method. The evidence obtained indicated that a concurrent or superimposed virus infection accelerated the tuberculous process in mice. The authors consider that the microscopic evidence suggests that the activity of small virus inocula alters the physiological state of the lung so as to make it more easily invaded by the tubercle bacillus, and that these inocula may be so small as to induce little or no virus pneumonia.

Protective Antigens from Brucella Abortus.

J. S. PATERSON, N. W. PIRIE AND A. W. STABLEFORTH (*The British Journal of Experimental Pathology*, June, 1947) have studied protective antigens isolated from *Brucella abortus*. The method of preparation of the antigen is described in detail and its chemical and physical properties are analysed. Its inoculation into rabbits intradermally produced pronounced local necrosis, and later a rise in antibody titre. Intramuscular inoculation avoids the production of a local lesion. The animals were protected from subsequent infection, but not in any significant degree was one dose of this antigen better than the American vaccine known as "S19".

Mechanism of Recovery from Pneumonia due to Friedländer's Bacillus.

LLEWELLYN SALE AND W. BARRY WOOD, JUNIOR (*The Journal of Experimental Medicine*, September, 1947), have used the white rat to study the mechanism of recovery from pneumonia due to Friedländer's bacillus. The infection appeared to be uniformly fatal after intrabronchial inoculation of bacilli suspended in mucin. Histological studies in various stages of the disease showed greater proliferation of organisms than is usually seen in pneumococcal pneumonia, organization of the exudate was common, and abscess formation often occurred. Phagocytosis of the bacilli was seen and spread to other parts of the lung followed the spread of œdema fluid to other alveoli.

LLEWELLYN SALE, MARY RUTH SMITH AND W. BARRY WOOD, JUNIOR (*ibidem*), have studied the effect of sulphonamide chemotherapy upon the pulmonary lesions of experimental Friedländer's bacillus pneumonia. Treatment by sulphadiazine or sulphamerazine was begun at intervals of from six to eighteen hours after the animals had been inoculated. Six hours after treatment had begun 50% of the animals had bacteriæmia. When treatment was began early 94% of the animals survived, but when it was delayed for eighteen hours none lived. Study of histological material from rats killed at various times during treatment showed that the drug prevented multiplication of the bacilli in the œdema-filled alveoli, and thereafter the lesion ceased to spread, phagocytosis became evident, some areas healed, while a few went on to abscess formation. The phagocytic process had no relation to the appearance of antibody in the blood.

MARY RUTH SMITH AND W. BARRY WOOD, JUNIOR (*ibidem*), have studied the role of surface phagocytosis in the destruction of the microorganisms in the lung. The techniques developed in an earlier study on the pneumococcus were used again, and once again established the fact that phagocytosis of Friedländer's bacilli takes place in the lung in the absence of antibody when conditions are such that the leucocytes have access to a suitable surface such as the alveolar wall against which to trap the bacteria. By direct observation and by a method of microculture it was shown that the viability of the ingested organisms was lowered and often destroyed. The authors discuss the reasons for the occurrence of localized abscesses in the pneumonia due to Friedländer's bacillus.

HYGIENE.

The Control of House Flies by DDT.

W. C. BAKER, H. I. SCUDDER AND E. L. GUY (*Public Health Reports*, April 25, 1947) experimented with DDT as a residual insecticide in the control of house flies. The following solutions were used. (i) A xylene-DDT-emulsifier concentrate consisting of three pounds of DDT (technical grade), three quarts of xylene (industrial grade), and six fluid ounces of triton X-100. This made about a gallon of 35% DDT concentrate. (ii) Wettable DDT consisting of equal parts of DDT and an inert powder. This formed a suspension with water and had to be agitated during use. Hand or mechanical air pressure sprayers were used with nozzles which produced a fan-shaped spray that was varied with the type of surface that was treated. The aims of the treatment were: (*a*) to use sufficient spray to wet the surfaces without causing the solution to run together and run off the surface; (*b*) to use sufficient spray to leave a deposit of 200 milligrammes

or three grains of DDT crystals per square foot. This for an area of 500 square feet is approximately equivalent to ten gallons of 0·5% solution, two gallons of 2·5% solution, one gallon of 5·0% solution, or three-quarters of a gallon of 7·5% solution. Particular care was taken to spray the resting places of the flies, such as suspended strings and wires, edges of projections and cracks in the ceiling. Those resting places were indicated by accumulations of fæcal fly spots. In dairies a complete spraying of walls and ceilings, with particular attention to all the edges of overhead woodwork on which the flies might rest, was carried out with 2·5% DDT solution. This controlled the house flies in the dairies for three months. The spray was applied at the rate of 200 milligrammes of DDT to the square foot. In restaurants it was found that a 7·5% solution was necessary to get the 200 milligrammes of DDT per square foot onto the high-gloss painted surfaces without causing the droplets on the surfaces to run together. The ceiling, walls and upholstery were sprayed with this solution. Cover clothes were used to prevent the spray from getting onto table-tops, food, cooking utensils and food preparation tables. One spraying by this method controlled the flies in the treated building for as long as four months. When DDT (200 milligrammes to the square foot) was applied as a cover spray in a 0·5% solution to fly-breeding areas, such as rubbish dumps and heaps of decaying food refuse, the hatching flies were controlled for intervals of approximately four weeks. If the adjacent buildings and structures were sprayed with a 2·5% DDT solution the flies were further controlled. In small premises it was found that flies could be controlled by hanging string treated with DDT from the ceiling in places accessible to flies. Net cord was soaked in a 35% solution of DDT and hung out to dry. Suitable lengths were then hung from the ceiling or suspended from shelves, light brackets, or other suitable articles where they would be accessible to flies.

Radioactive Emanations.

BRADFORD N. CRAVER (*The Journal of Industrial Hygiene and Toxicology*, May, 1947) states that the industrial use of X rays and radioactive materials is increasing and that there may be an industrial use for atomic energy. The radiations of practical consequence associated with radioactive material are α, β, γ and X rays and neutrons. Alpha rays can be stopped by a thin sheet of paper and protection from such rays should be simple Beta rays can be stopped by a sheet of aluminium two millimetres thick. Gamma and X rays are electromagnetic waves with great penetrating powers and similar effects. A centimetre or more of a heavy metal or a layer of concrete several feet thick is necessary to stop them. A neutron is stopped or slowed by collisions with a nucleus or an electron of an atom. When it is finally stopped it combines with a nucleus to cause the emission of a proton, α particle or one or more neutrons. A thickness of an aqueous solution of sodium borate is excellent protection against neutrons. It is rich in hydrogen atoms which slow the neutrons and boron atoms which ultimately capture them. The amount of radiation can be measured. The biological effects produced by radioactive emanations are believed to be due to the direct and indirect ionization produced in their passage through the tissues. The resulting ions produce chemical changes which alter the reactivity or the future of the containing cell. If a single chromosome is permanently changed by alteration, reorientation, or destruction, a mutation will be produced and the fate of future generations of cells immediately affected. Hæmatopoietic tissue whose reproductive or mitotic rate is high and reproductive tissue whose product is immature are very sensitive to radiation. The major cause of delayed deaths due to the radiational effects of the atomic bomb was reported to be a decrease in the white cells of the blood. An overwhelming dose of radiation as from an atomic bomb burst may cause death by the immediate cellular destruction. In cases in which the exposure to radiations from radioactive material is less intense there is a characteristic latent period of many hours to several days before symptoms appear. The effects of chronic exposure to doses of radiation slightly above the limit tolerable without injury vary from chronic dermatitis to neoplastic change. The incidence of neoplastic change in the blood-producing tissues in a group exposed to X rays was about eight times that amongst an unexposed group. The injurious effects of ingestion and inhalation of radioactive material have been studied in the case of radium dial painters, in the miners of radioactive material at Schneeberg and Joachimsthal and experimentally in animals. The metabolism of radium is similar to that of calcium and most of the residuum is found in the skeleton. A residuum of 0·5γ of radium could apparently be tolerated without symptoms. A residuum of 0·5γ to 2·0γ proved fatal, often because of the development of osteogenic sarcoma. The accepted limit of tolerance for radioactive material in the body is 0·1γ. This amount is such that in it $3{\cdot}7 \times 10^{10}$ atoms disintegrate per second, without regard for the type of radiation produced. For X-ray workers the accepted limit of tolerance is a total daily exposure of 0·1 Röntgen. A Röntgen is defined as that amount of radiation which will produce in one centimetre of air at a temperature of 0° C. and a pressure of 760 millimetres of mercury air ions carrying one electrostatic unit of quantity of electricity of either sign.

Incidence of Poliomyelitis in 1946.

C. C. DAUER (*Public Health Reports*, June 20, 1947) analyses the high incidence of poliomyelitis in the United States of America during 1946. The increase was principally in the north central part of the country. This increase may be due to one or all three of the following factors: an actual increase of the incidence in the various areas; a more widespread distribution of the infection; a more complete reporting of cases, particularly those of the non-paralytic group. Since there has been a rapid increase in the ratio of reported cases to deaths, the last two factors are considered as being probably responsible for the increase in the reported cases of poliomyelitis. During 1946 in Canada an epidemic of poliomyelitis commenced in the provinces of Quebec and Ontario and reached its peak in September. In the same year poliomyelitis was also more prevalent in other parts of the Western Hemisphere and epidemics occurred more frequently in the tropical regions than previously. From data available it appeared that the disease was milder in the tropics than in temperate regions. The author states that, in regard to the dissemination of the poliomyelitis virus, convincing epidemiological evidence is still lacking to support transmission of the virus by food, water, insects or arthropods. Investigations indicate that the virus can be recovered from the secretions of the oropharynx during a period not exceeding four days after the onset of symptoms. The virus has been recovered from pools of tonsils removed in the intervals between epidemics. Investigations indicate that an infected person may transmit the virus during a period three days before and four days after the onset of symptoms. A patient who had been in contact with an infected person during this period did not develop symptoms for nineteen days after the poliomyelitis virus was recoverable from his fæces. Accumulations of epidemiological data apear to have strengthened the hypothesis that poliomyelitis is spread principally by person-to-person contact and it is probable that the majority of infections are transmitted by persons who exhibit no recognizable symptoms of the disease.

Silica Dust.

ACCORDING to F. R. Holden, W. C. L. Hemeon and E. C. Hyatt (*The Journal of Industrial Hygiene and Toxicology*, July, 1947) it has been shown that the toxicity of silica particles in industrial dusts increases strikingly with decreasing size. The fibrosis-producing size is below 5·0μ. The most significant size range is from 1·0μ to 3·0μ. The composition of airborne dust may differ markedly from that of the source material from which it arises, and atmospheric industrial dust includes a considerable portion by weight of particles too large to be significant in silicosis production. To obtain a significant measure of the silicosis potentialities of any dust, the silica content of the fraction of dust which contains the small particles should be determined. A method is described for the collection and separation of the airborne dusts to eliminate the oversize particles before analysis. It consists briefly of the collection of dust by a sampling apparatus, salicylic acid being used as the filtering medium. The dust is separated from the salicylic acid by solution in ethyl alcohol and filtration. The dust is then suspended in alcohol and allowed to settle for forty minutes. The supernatant fluid is drawn off to a mark 3·3 centimetres below the original surface of the fluid. This procedure is repeated until the supernatant fluid is clear. All the supernatant fluid drawn off contains the fine fraction of dust.

Survival of Smallpox Virus.

A. W. DOWNIE AND K. R. DUMBELL (*The Lancet*, April 26, 1947) have found that the virus of smallpox, present in diluted vesicle fluid dried on glass slides, is capable of survival for five weeks when exposed to daylight and for twelve weeks when kept in the dark at room temperature. Virus was recovered from different samples of crusts kept at room temperature for several months and from one specimen kept for over a year.

Medical Societies.

MELBOURNE PÆDIATRIC SOCIETY.

A MEETING of the Melbourne Pædiatric Society was held on July 9, 1947, at the Children's Hospital, Carlton, Melbourne, DR. JOHN COLQUHOUN, the President, in the chair. Part of this report appeared in the issue of January 31, 1948.

Pyohydronephrosis.

DR. H. BOYD GRAHAM and DR. BRUCE HALLOWS showed a boy, aged nine years, suffering from pyohydronephrosis.

Dr. Boyd Graham said that he and Dr. Hallows wished to interest the members of the society in certain problems associated with the investigation and treatment of enuresis and pyuria as illustrated from a case history. He had arranged with Dr. Hallows to take over the presentation of the facts and the surgical problems from the point at which he had commenced special investigation of the urinary tract.

The boy had been enuretic all his life at night time and had poor control during the day, though the control had seemed to be improving for a few months prior to his admission to the Children's Hospital on May 8, 1947. He had been lacking in vitality during the year preceding that date, and in general his health had been poor; he had been losing weight for three months. On several occasions he had had attacks of pain in the left loin, and for two weeks prior to his admission to hospital he had been kept in bed, as the evening temperature ran approximately to 100° F. and his doctor had identified pus cells in the urine.

On examination of the child at the hospital, it was noted that the lower poles of the kidneys were palpable. The urine regularly contained pus cells, and the presence of *Staphylococcus aureus* was established in prepared smears and cultures. The boy was in an anæmic state of moderate degree. The hæmoglobin percentage was estimated at 72% (Sahli); the red cell count was 3,700,000 per cubic millimetre; the leucocyte count was 14,000 per cubic millimetre, the differential count revealing 78% of polymorphonuclear cells, 20% of lymphocytes, 1% of monocytes and 1% of eosinophile cells; no abnormal appearances were reported on examination of blood films.

Dr. Graham went on to say that from ordinary skiagrams of the abdominal area it was considered that the kidney outlines were normal in size, shape and position; but there was a vague opaque area overlying the left kidney shadow, which might represent a calculus. The report included the statement that the splenic shadow was not larger than it should be. Sulphadiazine tablets, two every four hours, were given to the boy regularly at first, and within three days the temperature fell to normal and fewer pus cells per high-power field were found in the urine. Excretion pyelography was carried out on May 22. No sign was present of the opacity in the left renal area in the previous films. The right kidney excreted dye within five minutes, and showed what appeared to be early clubbing of calyces and enlargement of the pelvis with kinking of the ureteric junction during inspiration. The left kidney shadow for the most part was obscured by bowel gas, and its contents were evident only after thirty minutes. A febrile exacerbation had occurred prior to the pyelographic examination, during which red blood cells had been found in the urine. The sulphadiazine therapy was on that account replaced by sulphaguanidine tablets given three every four hours. Ferrous sulphate and potassium citrate were being used in mixtures all along, and were continued.

Dr. Graham then said that, as it had been shown that there was an organic basis in the urinary tract for the boy's symptoms, the assistance of Dr. Hallows was invoked for cystoscopic and retrograde pyelographic examinations, to obtain a clearer diagnosis and data for indications for effective treatment. To assist him the excretion pyelographic examination was repeated; it was again found that the right kidney was excreting well, but on this occasion the shadows of the calyces, pelvis and ureter were not abnormal; on the left side, however, concentration was not so good, and the outline of the ureter was poorly defined, though it could be seen that the pelvis was enlarged and bifurcated and that the calyces were uniformly dilated. At that stage Dr. Graham asked Dr. Hallows to continue the story.

DR. BRUCE HALLOWS described the cystoscopic and retrograde pyelographic examinations carried out by him on June 18. He found gross hydronephrosis on the left side. In urine from the right ureter some red blood cells only were present, probably traumatic in origin. In urine from the left ureter there were numerous pus cells and some red blood cells, and a staphylococcus was grown on culture. From June 18 to June 28 penicillin injections were given, 10,000 units every three hours. As there was a shortage of penicillin, and as the number of pus cells had been reduced to only one or two per high-power field, the penicillin therapy was discontinued. At this stage a few colonies of *Staphylococcus albus* only were grown on culture from the urine. Dr. Hallows said that discussion was invited on the need for further investigation and on the relative merits of medical and surgical plans for treatment.

DR. GWEN VILLIERS said that the radiologist could be of considerable help in these cases. It had been shown radiographically that the function of the left kidney was non-existent and that of the right kidney was impaired. If it was possible to inject twenty millilitres of dye into the right side, it could be argued that the right kidney was abnormal and the condition was therefore bilateral, unless, of course, retrograde pyelography had been carried out under general anæsthesia. So from the radiological viewpoint kidney function tests did not warrant surgical interference.

DR. J. W. GRIEVE said that it was necessary to determine whether the condition was bilateral or not. In the vast majority of cases it was. The vast majority of patients also had increased blood urea contents, and the levels grew progressively higher. One was always hopeful of meeting a patient with the abnormality on one side only, to submit to surgical treatment. With the present X-ray findings it would appear that Dr. Graham's patient was not suitable for surgical treatment especially if the blood urea level was found to be high. Dr. Grieve said that he had observed cases of chronic pyuria with similar X-ray findings. There existed in such cases a tendency to precipitancy of micturition by day and to enuresis by night. A number of similar observations were met with in the literature.

DR. ROBERT SOUTHBY complimented Dr. Graham on the diagnosis. He said that often cases of bed-wetting were met with in the out-patient department, in which sedatives had been ordered without the child's being submitted to further investigation. It was essential to go closely into the history. If frequency of micturition during the day accompanied the bed-wetting, suspicions should be aroused. If the ward tests for albumin and sugar gave negative results, microscopic examination of the urine was proceeded with. If pus was found, the story was almost pathognomonic of the type of case described by Dr. Graham. Dr. Southby went on to say that the left kidney had been shown to be grossly hydronephrotic and secondarily infected, whilst the right side was apparently still clear of infection. The outlook might be improved by left nephrectomy.

DR. KEITH HALLAM said that pyeloscopy should be employed in all cases in which a retrograde urographic examination was made. It was just as necessary as fluoroscopy during a barium meal examination, in which the danger of pushing a sharp instrument into an organ did not exist. If the instrument was pushed too far, damage might easily accrue to a kidney. A urologist's education was incomplete if he had not directly viewed the renal pelvis. Dr. Hallam asked whether the retrograde pyelogram was prepared with the tube under the table. If so, the size of the calyces would be increased by 40% to 50%, and their true size would therefore have to be 50% or more less than that shown in the films. The amount of iodine injected had nothing to do with the case, since the dye readily flowed back into the bladder, and only a little might be injected into the renal pelvis. It was important not to be misled by films taken with the tube under the table. This distorted and magnified the pictures obtained, giving the appearance of hydronephrosis when it did not exist.

DR. IAN WOOD said that bacteriology had faded into the background in these cases. A physician had not completed his case till he had called in the surgeon to carry out retrograde pyelography. This threw more light on the condition than excretion pyelography. It was essential to determine whether both kidneys were involved. If the condition was unilateral and the radiologist was satisfied as a result of dye tests that the right kidney was normal, Dr. Wood said he would advocate left nephrectomy. Dr. Wood went on to say that it was impossible to determine the size of the bladder from one X-ray picture. Finally, Dr. Wood said that it was well known that patients with chronic pyelitis became anæmic. This might be alleviated by drug therapy directed against the infecting organism. It was a pity that penicillin was in short supply. When anæmia was controlled, the patient was subsequently in a position for nephrectomy.

DR. P. CODY said that he was not clear on one of Dr. Hallam's points. If the majority of patients were radiologically examined with the tube under the table, it should

be possible to determine the presence or absence of enlargement of the pelvic shadow by comparison with other films from normal subjects.

DR. REGINALD WEBSTER said that in the museum of the Children's Hospital there was a genuine specimen of unilateral hydronephrosis and hydroureter—an analogue of Hirschsprung's disease of the bowel.

DR. J. G. WHITAKER said that he agreed with Dr. Hallam's view that it was possible slowly to squirt in as much iodide as one liked, the solution returning to the bladder. Dr. Colin Macdonald had not furnished his report on the radiological features. The condition was not necessarily bilateral hydronephrosis and it was difficult to interpret the films.

Dr. Hallows, in reply, said that he had consulted Dr. Colin Macdonald on both films (excretion and retrograde pyelography). Dr. Macdonald had insisted that the right kidney was normal. He had reached this view, taking into regard the fact that the preparation of the films was carried out in the routine way. Although the right pelvis appeared to be dilated, the type of calcyces was normal. Dr. Hallows agreed with Dr. Hallam on the point that distortion arose from variation in the distance of the tube from the patient. Concerning the quantity of dye injected, he wished to make it clear that it was not all injected at the one time, and therefore could not be used as an indication of the size of the renal pelvis. With regard to the point raised by Dr. Wood on the size of the bladder, Dr. Hallows said that during the preliminary filling of the bladder for cystoscopic examination, twelve ounces of water were used. The bladder then felt moderately distended and could be palpated half way to the umbilicus. Dr. Hallows said that the right kidney appeared to him to be larger than normal, and he had the impression that the condition was bilateral. Dr. Colin Macdonald, however, was against this view. He had agreed that there might be some hydroureter on the right side, but that was as far as he was prepared to go. With regard to treatment, the fact that the number of pus cells in the urine had been reduced to one per high-power field encouraged temporizing; but if the condition was finally proved to be unilateral, surgical treatment should be instituted.

Dr. Graham, in reply, laid emphasis on teamwork in these cases. He said that the physician required help from the radiologist, the urologist and the resident medical officer. The discussion had topped this work, as it were. Renal function tests must be carried out. If surgical treatment was eventually indicated, the patient was to be made fit for it. Dr. Webster had said that the condition might be unilateral; Dr. Macdonald said that radiologically it was unilateral, and the concentration of iodine was good on the right side. A defeatist attitude must not be adopted. If the condition was bilateral, the right side would become as bad as the left and there would be little hope for the boy. Dr. Graham said that all this encouraged him to enlist the aid of the surgeon to remove the left kidney.

British Medical Association News.

THE OPHTHALMOLOGICAL SOCIETY OF AUSTRALIA (BRITISH MEDICAL ASSOCIATION).

THE seventh annual general and scientific meeting of the Ophthalmological Society of Australia (British Medical Association) was held at the Royal Australasian College of Physicians, Macquarie Street, Sydney, on September 23, 1947, DR. DARCY A. WILLIAMS, the President, in the chair. Those present included three members from Queensland, thirty-seven from New South Wales, thirteen from Victoria, two from Tasmania, four from South Australia and one from Western Australia.

President's Address.

The President, Dr. Darcy A. Williams, after welcoming such guests as had been able to attend, addressed members upon the subject of early ophthalmologists in Australasia and upon the association of ophthalmologists with Australasian medical congresses. Amongst the early ophthalmologists mentioned by the President were James T. Rudall, Andrew S. Gray, T. Aubrey Bowen, Charles Grosse, Thomas Evans and W. Odillo Maher.

The gradual growth of ophthalmology was traced and the manner in which it kept pace with changes overseas was described. An endeavour was made to salvage from the past valuable material and interesting and at times amusing anecdotes.

Research Technique and Training Overseas.

DR. N. M. MACINDOE (Sydney), who had recently returned from a visit to Europe and the United States of America, where he had made close inquiries into subjects of ophthalmological interest, read two papers, accompanied by fifty illustrations, describing recent advances in research and technique, and trends in organization and training abroad. In Boston research was concerned with the subject of steamy cornea, optic neuritis and the visual acuity of moving objects; at Johns Hopkins University with virus diseases and sympathetic ophthalmia; in New York with colour vision; in London with paretic strabismus; in Copenhagen with colloidometry; and in Stockholm with chromatic light and electroretinograms. In Zurich work was being done on corneal distortion, subjective screens for central scotomata, perimetry, slit lamp examination of the fundus, the context of the pathological aqueous humour, tonometry and ciliary permeability. New technique described included gonioscopy, drug treatments, the use of radioactive iodine, scleral resection in myopia, corneal grafting, methods of treating glaucoma and cataract and an operation for retinal detachment.

The section on training was a reasoned appeal for more apprenticeship and fewer academic examinations. The continental clinic system was described and the suggestion put forward that we should copy the American type of organization in our big centres.

An Outline of Orthoptics in New South Wales.

MISS E. RUSSELL read a composite paper expressing the general opinion and experience of five senior practising orthoptists. It was pointed out that the practice of orthoptics by orthoptists had begun in Sydney in 1933. At the present time eleven orthoptists were working in hospitals, intermediate clinics and private practice, and an average of 576 treatments were given each week.

In the early years squint had been the condition treated in the great majority of cases, prisms being used for phorias as a general means of relieving or attempting to relieve symptoms. While the treatment of strabismus went on in growing numbers, the realization of the great benefit given by orthoptic treatment to those with phorias and allied conditions had widened the field amazingly. Phorias of all types were receiving increased attention since it was in these cases that distress might be acute. Orthoptists felt that their greatest battle was still with strabismus, and it was there that they did their toughest job. They had learnt during the years to assess the relative possibilities of success with the different types. With all types suppression was their greatest enemy.

In regard to convergent squints, true accommodative conditions responded to treatment only and borderline squints needed surgical help. Tonic squints required orthoptics before and after surgery. Congenital conditions were often successfully treated.

The treatment of divergent squint was less difficult. In all cases of strabismus orthoptic investigation was important before surgery to assess the binocular condition; in other words; abnormal projection *et cetera*. Suppression was their greatest enemy. Occlusion was their greatest ally. Treatment had been speeded up generally. There would always be conditions, difficult in themselves, and rendered more so by mental dulness, inability to concentrate and frail health on the part of the patients and indifferent parental cooperation, in which treatment would be lengthy, but which eventually could be cured or greatly alleviated. As long as definite progress was being made they felt that treatment should continue.

The Selection of Patients for Orthoptics.

DR. JOHN ANTILL POCKLEY (Sydney) indicated some factors in the selection of cases for orthoptics. It was concluded that the personality of the patient and parent was of first importance in the selection of many cases and that amongst adults many phorias arose from psychogenic causes. It was desirable to aim at the earliest possible cure, and, when conditions were suitable, operation should be early. Waiting for young children to reach orthoptic age was undesirable. A close liaison between ophthalmic surgeon and orthoptist was essential.

Two Cases of Chorioidal Melanomata with Unusual Histories.

DR. KEVIN O'DAY (Melbourne) read a paper on two cases of intraocular tumours of unusual interest.

The first was a malignant melanoma of the chorioid. Four years before the eye was finally removed, the patient had been advised by four ophthalmic surgeons that removal was

necessary because of a malignant tumour. Even after four years the tumour was not very large. The eye was slightly inflamed, but there was no evidence of glaucoma. Very little pigment was present in the tumour. There was a metastatic nodule at the root of the iris. The tumour was of mixed-celled type with many thin-walled blood sinuses. Mitotic figures were very difficult to demonstrate. Wilder's stain demonstrated dense reticulin in the base of the tumour, with very little at the summit where it had broken through Bruch's membrane. Wilder was of the opinion that the prognosis was better in tumours with a dense reticulin. The speaker suggested that this stain should be used because of its probable prognostic significance in every case in which the eye was excised.

The second case was a retinoblastoma in a man of twenty-nine years, an unusual age for this tumour. The patient gave a history of loss of vision and pain in the eye after an injury at football. When he was first seen the vision was reduced to appreciation of hand movements in the temporal field. The tension was raised and the anterior chamber full of blood. After cyclodialysis the pain and the tension were relieved and the man went back to the country. He returned a year later with the history of a further blow at football, and the eye completely blind and painful. The tension was raised and the eyeball enlarged with a ciliary staphyloma on the temporal side. The anterior chamber was full of blood. The eye was excised. When the eye was opened, the tumour was found to involve the retina, the iris, the ciliary body and the anterior chamber. A cyclitic membrane was present and the tumour cells were growing along this. A portion in the ciliary body was anaplastic, but sections of the retina demonstrated the mantles of cells surrounding vessels, typical of retinoblastoma. Actively growing areas alternated with necrotic ones. There was no evidence of extraocular extension, although mitotic figures were very numerous.

The Treatment of Dendritic Ulcer.

DR. FRANK PHILLIPS (Hobart) described his treatment for dendritic ulcer, which included removal of the damaged tissue with blotting paper and the use of a dental drill on the more resistant areas of ulceration. He maintained that his treatment was better than the customary methods.

Endothelial Dystrophy.

DR. CLIFFORD S. COLVIN (New South Wales) described a case of endothelial dystrophy in a female patient, aged sixty-two years. Improvement followed glandular and vitamin therapy. Dr. Colvin concluded his paper with a discussion on the condition.

Dry Eyes.

DR. J. BRUCE HAMILTON (Hobart) read a paper on the subject of *keratoconjunctivitis sicca*, amplifying his paper read before the society in 1940 and published in Volume II of the society's *Transactions*. He reported another twenty-four cases and described the treatment employed, which had had generally satisfactory results. Dr. Hamilton discussed the differential diagnosis of the condition and its relation to Mikulicz's disease, Plummer-Vinson syndrome and iodide mumps. He pointed out that the disease was of great interest in its possible association with other common morbid processes.

Allergic Sensitivity to Procaine Hydrochloride and the Hypodermic Injection of Cocaine Hydrochloride.

DR. ARTHUR D'OMBRAIN (Sydney) dealt with the characteristics of drug allergy and described a harrowing personal experience of dermatitis due to procaine. He maintained that collapse from cocaine injection was really an outright poisoning and not an anaphylactic reaction. He related personal experience in the use of injection of 0·75% solution of cocaine hydrochloride over a period of six years. Dr. D'Ombrain's opinion was that cocaine could be injected with safety, provided a correct concentration and dosage were used.

Eye Protection in Industry.

DR. A. L. TOSTEVIN (Adelaide) stressed the necessity for greater cooperation between oculists, industrialists, lighting engineers, architects, the National Safety Council and the Standards Association not only in regard to the protection of eyes in industry, but also in the field of sight testing and correction of vision of employees. Examples were given of eye injuries in the various large industries and reference was made to the International Chemical Industries Safety Council as a guide to industry towards protecting the worker and improving his efficiency. Emphasis was placed on the fact that most of the metal industries showed that 50% of the lost time from accidents was due to eye injuries. Dr. Tostevin surveyed the research work being carried out in Australia and also the measures to be taken in prevention of eye injuries generally.

Partial Thrombosis of the Central Retinal Vein Treated with Dicoumarin.

DR. R. G. BANKS-SMITH (New South Wales) read extracts from available literature on the subject of the treatment with dicourmarin of thrombosis of the central retinal vein. The types of the condition were described and indication was given of those types in which treatment might be successful. Dr. Banks-Smith submited a case report in which partial success was claimed and he issued a warning as to the possibilities of overdosage.

Radon Treatment of Intraocular Tumour.

DR. W. LOCKHART-GIBSON (Brisbane) sketched a short history of the various methods of irradiation that had been used for the treatment of intraocular tumour, and drew attention to the value of active cooperation of the biophysicists with the radiotherapists in working out accurate dosage. He described the use of radon seed bent into the form of a circle with a spoke in it and moulded to the curve of the globe of the eye. Dr. Gibson claimed that such a seed was easy to suture into position, and could be arranged to give an accurate dosage to the tumour and as little irradiation as possible to those parts of the eye which were extremely sensitive to irradiation.

Charts of isodosage were exhibited and the treatment of a child with two tumours in its only eye was described.

The Lagrange Operation.

DR. J. A. O'BRIEN (Melbourne) presented a summary of sixty cases of Lagrange operation for glaucoma. He explained various modifications of technique. His results with complete or button-hole iridectomy were similar and his proportion of successful operations was 85%.

A Preliminary Survey of Ophthalmic Diseases in Western New South Wales.

DR. K. B. REDMOND (New South Wales) analysed 2000 case records of patients in western New South Wales, taking into account the geographical area of residence and environmental conditions of life and work. The area included part of the central tablelands, the central western slope and the western plains. Dr. Redmond concluded that factors of sunshine, dryness, glare and dust were important. He presented a table of ophthalmic diseases and their percentage incidence.

Non-Magnetic Metallic Intraocular Foreign Bodies.

DR. LENNOX PRICE (Newcastle) said that in civil practice it was unusual to meet a case of penetrating wound of the eye with a retained intraocular foreign body which was metallic and yet non-magnetic. Under war conditions, on the other hand, such cases occurred more commonly. Reports were given of two cases of this type which were treated within a few weeks of each other. The histories, clinical features and results in each case were compared and contrasted.

Optic Neuritis following Measles.

DR. T. C. MEURER (New South Wales) dealt with optic neuritis and post-neuritic optic atrophy following measles. He said that the condition was a result of an encephalomyelitis which occurred late in the course of measles usually when the rash was beginning to fade. The resulting optic atrophy did not appear to have any relation to the severity of the encephalomyelitis or the severity of the attack of measles. Three cases were reported and a review of the literature was presented.

Social.

The annual dinner was held at the Royal Automobile Club of Australia and was attended by 42 members and guests.

On the evening of the opening day a cocktail party was given at the Wentworth Hotel by the Ophthalmological Society of New South Wales.

Annual General Meeting.

The annual general meeting dealt with general business. Dr. Claude Morlet was elected President for the ensuing year and Dr. Arthur H. Joyce Vice-President.

It is proposed to hold the eighth annual general and scientific meeting at Perth in conjunction with the sixth session of the Australasian Medical Congress (British Medical Association), August 15 to 21, 1948.

Post-Graduate Work.

COURSE IN MEDICAL STATISTICS.

THE Post-Graduate Committee in Medicine in the University of Sydney announces that a course in medical statistics applied to experimental data in medicine will be held from February 12 to April 22, 1948. This course has been designed to give an introduction to workers in the medical sciences who have previously not had an opportunity to become familiar with statistical techniques. No attempt will be made to develop the mathematical side, since its purpose is chiefly to interest those new to the field. It is suggested that the course should be of value to those graduates who are just beginning research work in any field and should be of assistance in helping to systematize observations.

The programme consists of the following twelve lectures to be held on Thursday afternoons at 4 p.m. in the Council Room of the British Medical Association, 135, Macquarie Street, Sydney.

February 12 (Dr. H. O. Lancaster): The aims of statistics—description, classification and reduction of data, assessment of significance; an example on a hæmoglobin survey briefly discussed; the concept of frequency distribution.

February 19 (Dr. H. O. Lancaster): Systematic description of a population; mean, median, mode; measures of dispersion, (*a*) standard deviation and variance, (*b*) mean deviation, (*c*) range; symmetry and skewness.

February 26 (Dr. H. O. Lancaster): Graphic presentation of data; graphs—arithmetic, logarithmic and other grids.

March 4 (Dr. H. O. Lancaster): The normal distribution; introductory remarks on significance tests.

March 11 (Dr. H. O. Lancaster): The normal distribution continued; mortality rates and other ratios.

March 18 (Dr. H. O. Lancaster): Some remarks on tabulation; the treatment of fourfold tables with chi-square.

April 1 (Dr. H. O. Lancaster): Chi-square test used as test of goodness of fit, homogeneity *et cetera*; its use in statistical control of laboratory work.

April 8 (Dr. H. O. Lancaster): Bias, sampling selection.

April 15 (Miss Helen Newton Turner, of the Council for Scientific and Industrial Research): Analysis of variance.

April 22 (Miss Helen Newton Turner): Design of experiments.

Correspondence.

POLIOMYELITIS IN PREVENTIVE ORTHOPÆDICS.

SIR: In your journal of January 10, 1947, Dr. C. Craig has asked me two questions concerning the possibility of ice-cream acting as a means of spread of the virus of poliomyelitis. I would be grateful for space to reply.

The questions were: (i) Had I made quite certain that the milk used by ice-cream manufacturers is not pasteurized? (ii) If pasteurization is a routine practice in the manufacture of ice-cream, am I then satisfied or do I consider that there is still a possibility of spread by this means?

I prepared the article to which Dr. Craig refers knowing that the big manufacturers pasteurize their product, and I agree with the report made to Dr. Craig that it is an accepted practice that the milk used in the manufacture of ice-cream is pasteurized twice, first in the country and later after mixture with the other ingredients. At the second pasteurization one firm holds the mixture at 150° F. (65·5° C.) for thirty minutes, another firm holds the mixture at 145° F. (62·7° C.) for thirty minutes during which the temperature reaches 160° F. (69·9° C.) for ten minutes. Knowing these facts I am still not satisfied that ice-cream can be excluded from the possibility of acting as a vector of the virus of poliomyelitis. I am not satisfied for two reasons.

1. It has not been proven that the temperatures used in pasteurization are effective in inactivating virus when the virus is mixed with milk. I cannot find in the literature of poliomyelitis any record of experimental work to determine the temperature at which a strain of human poliomyelitis recently adapted to the monkey has been inactivated by exposure to heat when the infective material is mixed with ice-cream. Lawson and Melnick, of Yale University Medical School, published in *The Journal of Infectious Diseases* of March and April, 1947, a report of their work using murine poliomyelitis virus. Though I realize fully the dangers of argument from analogy, their findings and conclusions demand attention. Lawson and Melnick stated:

> Milk exerts a protective action on the destruction by heat of rodent adapted and spontaneous mouse poliomyelitis viruses when infected central nervous system tissue is heated in this medium. Milk enables these strains to withstand about 5° C. to 10° C. more heat for thirty minutes than when the infected tissues are suspended in water.
>
> When concentrated suspensions of infected nervous tissue are heated for thirty minutes, occasional traces of virus may be detected when the temperature is kept at 65° C. and 70° C. This is above the temperature commonly used in pasteurization (61·7° C.). However the number of variables which affect this type of experiment are so great that unqualified didactic statements about the thermo-stability of these agents cannot readily be made.
>
> No information about human (monkey adapted) poliomyelitis virus is furnished by these experiments. It is our belief that the knowledge of the survival of poliomyelitis virus in milk and ice-cream is inadequate.

The concluding sentence of the summary of Lawson and Melnick is important in its warning that reliance on pasteurization at the temperatures used at present rests on hope and assumption rather than proven facts. In view of the enormous consumption of ice-cream, it is important that the work of Lawson and Melnick should stimulate further research work on similar lines using human poliomyelitis virus recently adapted to the monkey. Until further knowledge has been obtained I would suggest that manufacturers of ice-cream anxious to reduce the possibility of their product acting as a vector, may consider heating the mixture to temperatures above 70° C. for thirty minutes.

The second and more important reason why I am not satisfied that ice-cream should be regarded as absolutely safe is the possibility of contamination after the pasteurization of the mixture. I understand that in Victoria the product has to conform to certain standards laid down by regulations administered by the health department: (*a*) standards relating to composition of the product to comply with the *Pure Foods Act*, and (*b*) standards of cleanliness.

I would suggest that in each State a survey should be made by medical officers of the health department to determine whether the standards of cleanliness are sufficient to exclude the possibility of contamination of the product after the second pasteurization has been completed.

For example, during the cooling process, is there any possibility of contamination by exposure to dust and flies? Is there any handling in the packing and wrapping of any of the various types of containers used for distribution, and especially, is there any possibility of contamination in retailing ice-cream from the bulk container to the consumer? Such a survey should include the methods adopted in the manufacture of ice-cream by all registered manufacturers including hotels and cafés and retailing methods generally.

As stated by Dr. Craig, the possibility of infection from milk can be overcome by careful parents by simply scalding the milk. The public have the right to the same feeling of security for ice-cream.

Yours, etc.,

41, Spring Street, JEAN MACNAMARA.
Melbourne,
January 25, 1948.

THE ROLE OF SPINAL FUSION IN ARTHROGENIC SCIATICA.

SIR: In reply to Dr. James H. Young I wish to supply the following information regarding occupations of the thirty-seven cases of arthrogenic sciatica reviewed in my article.

There were sixteen females and twenty-one males. Of the sixteen females thirteen were engaged in home duties, one was a clerk, one a bus driver and the remaining one was a trained nurse. The only one of these which was compensatable was the nurse. The bus driver could possibly have come under the category of compensable, except that she was running her own business and this did not arise.

Of the twenty-one male patients there were eleven labourers, two farmers, two gardeners, one fireman (railways), one lorry driver, one taxi driver, one fruitgrower and two salesmen. Except for the two salesmen these were all

laborious occupations. In seven of these the condition was allowed as compensatable, including one of the salesmen. Five more were repatriation cases, and as their disability had been considered to be due to, or aggravated by, war service they really come under the same category as compensation cases. Thus of the total of thirty-seven cases thirteen were compensatable.

Yours, etc.,
E. F. WEST.

Goldsbrough House,
172 North Terrace,
Adelaide.
January 21, 1948.

Australian Medical Board Proceedings.

QUEENSLAND.

THE undermentioned have been registered, pursuant to the provisions of *The Medical Acts, 1939 to 1946*, of Queensland, as duly qualified medical practitioners:

Darby, Leonard, M.B., B.S., 1912 (Univ. Melbourne), Tamar, Labrador, Southport.

Gazzard, Edward John, M.B., B.S., 1947 (Univ. Sydney), General Hospital, Brisbane.

Williams, Cyril Henry, M.B., Ch.B., 1923 (New Zealand), D.P.H., R.C.P. and S., 1932 (England), 107, Albion Street, Warwick.

Nominations and Elections.

THE undermentioned have applied for election as members of the New South Wales Branch of the British Medical Association:

King, Raymond Henry, provisional registration, 1948 (Univ. Sydney), 1A, Sandridge Street, Bondi.

Dunlop, Donald Buzzard, M.B., B.S., 1946 (Univ. Sydney), 6, Florence Street, Strathfield.

Dent, John Arthur Sherbon, provisional registration, 1947 (Univ. Sydney), Western Suburbs Hospital, Croydon.

Scott, Godfrey Chesworth, M.B., B.S., 1944 (Univ. Sydney), c.o. Dr. L. Bamber, Lithgow, New South Wales.

Obituary.

FREDERIC JOHN CHAPPLE.

WE regret to announce the death of Dr. Frederic John Chapple, which occurred on January 22, 1948, at Adelaide.

ALEXANDER LYONS.

WE regret to announce the death of Dr. Alexander Lyons, of Eaglehawk, which occurred on January 23, 1948.

WALTER HENRY DIVE.

WE regret to announce the death of Dr. Walter Henry Dive, which occurred on January 25, 1948, at Balgowlah, New South Wales.

ROBERT ANDREW DUVAL.

WE regret to announce the death of Dr. Robert Andrew Duval, which occurred on January 29, 1948, at London.

MARY ANKETELL BELL.

WE regret to announce the death of Dr. Mary Anketell Bell, which occurred in January, 1948, at Melbourne.

Books Received.

"Synopsis of Obstetrics", by Jennings C. Litzenberg, B.Sc., M.D., F.A.C.S.; Third Edition; 1947. St. Louis: The C. V. Mosby Company. Melbourne: W. Ramsay (Surgical) Proprietary, Limited. 7¾" × 4¾", pp. 416, with many illustrations, some of them coloured. Price: 41s.

"The Appendix", by R. J. McNeill Love, M.S. (London), F.R.C.S. (England), F.A.C.S.; 1947. London: H. K. Lewis and Company, Limited. 7¼" × 4¾", pp. 202, with many illustrations. Price: 12s. 6d.

Diary for the Month.

FEB. 6.—Queensland Branch, B.M.A.: Branch Meeting.
FEB. 10.—New South Wales Branch, B.M.A.: Executive and Finance Committee.
FEB. 12.—Victorian Branch, B.M.A.: Organization Sub-committee.
FEB. 13.—Queensland Branch, B.M.A.: Council Meeting.

Medical Appointments: Important Notice.

MEDICAL PRACTITIONERS are requested not to apply for any appointment mentioned below without having first communicated with the Honorary Secretary of the Branch concerned, or with the Medical Secretary of the British Medical Association, Tavistock Square, London, W.C.1.

New South Wales Branch (Honorary Secretary, 135, Macquarie Street, Sydney): Australian Natives' Association; Ashfield and District United Friendly Societies' Dispensary; Balmain United Friendly Societies' Dispensary; Leichhardt and Petersham United Friendly Societies' Dispensary; Manchester Unity Medical and Dispensing Institute, Oxford Street, Sydney; North Sydney Friendly Societies' Dispensary Limited; People's Prudential Assurance Company Limited; Phœnix Mutual Provident Society.

Victorian Branch (Honorary Secretary, Medical Society Hall, East Melbourne): Associated Medical Services Limited; all Institutes or Medical Dispensaries; Australian Prudential Association, Proprietary, Limited; Federated Mutual Medical Benefit Society; Mutual National Provident Club; National Provident Association; Hospital or other appointments outside Victoria.

Queensland Branch (Honorary Secretary, B.M.A. House, 225, Wickham Terrace, Brisbane, B.17): Brisbane Associated Friendly Societies' Medical Institute; Bundaberg Medical Institute; Brisbane City Council (Medical Officer of Health). Members accepting LODGE appointments and those desiring to accept appointments to any COUNTRY HOSPITAL or position outside Australia are advised, in their own interests, to submit a copy of their Agreement to the Council before signing.

South Australian Branch (Honorary Secretary, 178, North Terrace, Adelaide): All Lodge appointments in South Australia; all Contract Practice appointments in South Australia.

Western Australian Branch (Honorary Secretary, 205, Saint George's Terrace, Perth): Wiluna Hospital; all Contract Practice appointments in Western Australia. All government appointments with the exception of those of the Department of Public Health.

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